THE

BRITISH AND FOREIGN
MEDICO-CHIRURGICAL
REVIEW

OR

QUARTERLY JOURNAL

OF

PRACTICAL MEDICINE AND SURGERY.

VOL XII

JULY—OCTOBER, 1853

LONDON
JOHN CHURCHILL, 46, PRINCES STREET, SOHO,
AND
SAMUEL HIGHLEY, JUN, 32, FLEET STREET.

MDCCCLIII.

CONTENTS OF Nº XXIV

OF THE

BRITISH AND FOREIGN MEDICO-CHIRURGICAL REVIEW.

OCTOBER, 1853

Analytical and Critical Reviews

PAGE
REV I—1 De Partibus Similaribus By G FALLOPIUS (Date uncertain) 285
2 Theoria Generationis By CASPAR FRIEDRICH WOLFF 1759 ib
3 Theorie von der Generation By C F WOLFF 1764 ib
4 Theoria Generationis By C F WOLFF Ed Nova 1774 ib
5 Von der eigenthumlichen und wesentlichen Kraft der Vegetabilischen sowohl als auch der animalischen Substanz By C F WOLFF 1789 ib
6 Entwickelungs Geschichte d Thiere By K E VON BAER 1828 ib
7 Untersuchungen uber Phytogenesis By SCHLEIDEN 1837 ib
8 Mikroskopische Untersuchungen By SCHWANN 1838 9 ib
9 Ueber das Bindegewebe By REICHERT 1845 ib
10 Die Vegetabilische Zelle By H VON MOHL 1850 ib
The Vegetable Cell By H VON MOHL Translated by A HENFREY F R S ib
11 On the Mutual Relation of the Vital and Physical Forces By W B CARPENTER, M D, F R S, &c (Philosophical Transactions) 1850 ib
12 Handbuch der Gewebelehre By A KOLLIKER 1852 ib
REV II—1 Observations on Intestinal Obstructions depending on Internal Causes By BENJAMIN PHILLIPS, F R S ('Medico Chir Trans, vol xxxi) 315
2 Case of Stricture of the Colon, successfully treated by Operation, with an Analysis of Forty four Cases of Artificial Anus By CÆSAR HAWKINS, Senior Surgeon to St George's Hospital (Medico Chir Trans vol xxxv) ib
3 Remarkable Case of Foreign Bodies in the Stomach and Duodenum By JOHN MARSHALL Esq ('Medico Chirurgical Transactions, vol xxxv) ib
4 Two large masses of Hair and String matted together, taken from the Stomach and Duodenum By GEORGE POLLOCK ('Trans of Path Soc of Lond, 1852 53, p 329) ib
5 A Case of Constipation of Nine Days duration from Scirrhous Rectum By JOHN ADAMS Surg to the London Hosp (Medico Chir Trans,' vol xxxv) ib
6 An Account of Two Cases of Intestinal Obstructions By WILLIAM J CLEMENT, F R C S ('Medico Chirurgical Transactions, vol xxxv) ib
7 A Case of Intestinal Obstruction from Disease of the Rectum By ALFRED BAKER Surgeon to the General Hospital, Birmingham (Med Chir Trans, vol xxxv) ib
8 Case of Intestinal Obstruction By J LUKE Vice President of the Royal College of Surgeons of England, and Senior Surgeon to the London Hospital ib
9 Intestinal Obstruction of Twelve Days duration from Stricture of the Ascending Colon By ALEXANDER SHAW, Esq Surgeon to the Middlesex Hospital ('Transactions of Pathological Society of London, 1852 53) ib
10 Internal Strangulation of a portion of the Small Intestine by a strong Fibrous Band connected with the Mesentery By N WARD, Esq, Assistant Surgeon to the London Hospital ('Transactions of Pathological Society of London, 1852 53) ib

PAGE

REV III —A Treatise on Percussion and Auscultation By Dr JOSEPH SKODA Translated from the fourth edition by W O MARKHAM, M D 333

REV IV —Der Nervus Phrenicus des Menschen Eine Monographie Von Dr HUBERT LUSCHKA, Professor der Medizin an der Universität Tubingen 343
A Monograph on the Phrenic Nerve By Dr HUBERT LUSCHKA ib

REV V —1 Die Pathologischen Pigmente Von RUDOLPH VIRCHOW (VIRCHOW und REINHARDT'S 'Archiv fur path Anat' Vol I p 379, and 407 1847) 348
The Pathological Pigments By RUDOLPH VIRCHOW ib
2 Hæmatoidin und Bilifulvin Von R VIRCHOW ('Verhandlungen der Physikalisch Medicinischen Gesellschaft in Wurzburg, p 305 1850 ib
Hæmatoidin and Bilifulvin By R VIRCHOW ib
3 Beobachtungen uber eine eiweissartige Substanz in Crystallform Von K E REICHERT, in Dorpat ('MULLER'S Archiv p 197 1849) ib
Observations on an Albuminous Crystalline Substance By K E REICHERT ib
4 Ueber das Milzvenenblut Von OTTO FUNKE ('Zeitschrift fur rationelle Medicin, von Dr HENLE und Dr PFEUFER pp 172—218 1851 ib
On the Blood of the Splenic Vein By Dr O FUNKE ib
5 Neue Beobachtungen uber die Krystalle des Milzvenen und Fischblutes Von Dr O FUNKE ('Zeitsch fur rat Med Vol II p 198 1852) ib
Recent Observations on the Crystals of the Blood of the Splenic Vein and of Fishes By Dr O FUNKE ib
6 Ueber Blut Krystallisation Von Dr O FUNKE ('Zeitschrift fur rationelle Medicin von Dr HENLE und PFEUFER Vol II p 288 1852) ib
On Blood Crystallization By Dr O FUNKE ib
7 Ueber Krystallbildung im Blute Von Dr F KUNDE ('HENLE und PFEUFER'S Zeitschrift' Vol II p 271 1852) ib
On the Formation of Crystals in the Blood By Dr F KUNDE ib
8 The Formation of Crystals in Human Blood By L A PARKES, M D ('Medical Times and Gazette 1852) 349
9 Atlas der Physiologischen Chemie Von Dr O FUNKE ib
Atlas of Physiological Chemistry By Dr O FUNKE ib

REV VI —Ueber Geburtshulfe und Gynækologie in Frankreich Grossbritannien, und Irland grossentheils nach Reiseergebnissen Von Dr F H ARNETH 366
On Midwifery and Gynecology in France, Great Britain, and Ireland chiefly drawn from Observations made during Travels in those countries By Dr F H ARNETH ib

REV VII —1 Archiv des Vereins fur gemeinschaftliche Arbeiten zur Forderung der wissenschaftlichen Heilkunde Herausgegeben von Dr J VOGEL, Dr H NASSE, und Dr F W BENEKE Bd i, Heft 1 1853 380
Archives of the Society for United Labours towards the Advancement of Scientific Medicine Edited by Drs VOGEL NASSE, and BENEKE ib
2 Correspondenz Blatt des Vereins fur gemeinschaftliche Arbeiten zur Forderung der wissenschaftlichen Heilkunde 'Nos I to III 380
Corresponding Sheet of the Society for United Labours towards the Advancement of Scientific Medicine ib

REV VIII —1 Traité pratique des Maladies Vénériennes Par S G MAISONNEUVE, M D, et H MONTANIER, M D 385
Practical Treatise on Venereal Diseases, with a chapter on Syphilization ib
2 Traite des Maladies Vénériennes Par A VIDAL (DE CASSIS) ib
Treatise on Venereal Diseases ib
3 Report à M le Préfet de Police, si M le Dr Auzias Turenne peut être autorise à appliquer ou à experimenter la Syphilisation? Par MM les Docteurs MELIER, PHILIPE RICORD DENIS CONNEAU, et MARCHAL (DE CALVI) ib
Report to the Prefect of Police, on the question whether Dr Auzias Turenne be permitted to practise or experiment on Syphilization? ib
4 Syphilitic Diseases, their Pathology, Diagnosis, and Treatment, including Experimental Researches on Inoculation By JOHN EGAN, M D M R I A ib
5 Observations on Syphilis, and on Inoculation as a means of Diagnosis By JOHN CROWCH CHRISTOPHERS, M R C S ib
6 Traité practique des Maladies Vénériennes Par le Dr PHILIPE RICORD ib

PAGE
2 La Sipilizzatione studiata qual mezzo curativo e preservativo delle Malattie Veneree De Cusimino Sperino 38
Syphilization treated as a Curative and Preventive in the Venereal Disease ib
Rev IX —Practical Observations on Diseases of the Lungs and Heart By Archibald Billing, M D, F R S, &c &c 398
Rev X —Handbuch der Diatetik Von Jac Moleschott, M D 402
Manual of Dietetics By Dr Moleschott ib
Rev XI —1 Histoire Naturelle des Vegetaux Parasites, qui Croissent sur l'Homme, et sur les Animaux Vivants Par Charles Robin, M D, &c (Avec un Atlas de 15 planches gravees) ib
Natural History of Parasitic Vegetables By Charles Robin M D ib
2 Recherches sur la Nature et le Traitement des Teignes Par M E Bazin, Medecin de l'Hopital St Louis ib
Researches into the Nature and the Treatment of Tineæ By M E Bazin M D ib
Rev XII —1 A Bill intituled An Act to amend an Act passed in the Ninth Year of Her Majesty, for the Regulation of the Care and Treatment of Lunatics 424
2 A Bill intituled 'An Act to consolidate and amend the Laws for the Provision and Regulation of Lunatic Asylums for Counties and Boroughs, &c ib
3 A Bill intituled 'An Act for the Regulation of Proceedings under Commissions of Lunacy, and the Consolidation and Amendment of the Acts respecting Lunatics and their Estates ib
Rev XIII —Norsk Magazin for Lægevidenskaben Heft 1—10 1852 434
Norse Magazine for Medical Science Parts 1 to 10 ib
Rev XIV —1 A Treatise on Tuberculosis, the Constitutional Origin of Consumption and Scrofula By Henry Ancell, late Surgeon to the Western General Dispensary, &c &c 443
2 The Nature, Symptoms, and Treatment of Consumption By Richard Payne Cotton, M D, Member of the Royal College of Physicians, and Assistant Physician to the Hospital for Consumption ib

---

Bibliographical Record.

Art I —The Transactions of the American Medical Association Vol V 453
Art II —Anatomical and Physiological Bibliography, from 1849 to 1852 inclusive By G E Day, M D, Professor of Medicine in the University of St Andrews ib
Art III —Lectures on Surgical Pathology, delivered at the Royal College of Surgeons By James Paget F R S 454
Art IV —Researches on the Primary Stages of Histogenesis and Histolysis By Robert D Lyons, M B, &c ib
Art V —The Harmonies of Physical Science, in Relation to the Higher Sentiments, with Observations on the Study of Medical Science By William Hinds, M D 456
Art VI —Commentaries on the Surgery of the War in Portugal, Spain, France, and the Netherlands By G J Guthrie, F R S Fifth Edition, revised to 1853 459
Art VII —On the Nature and Treatment of the Deformities of the Human Frame By W J Little, M D ib
Art VIII —On the Use of an Artificial Membrana Tympani in cases of Deafness, dependent upon Perforation of the Natural Organ, to which is added a paper, entitled, Ought the Tonsils to be Excised in the Treatment of Deafness? By Joseph Toynbee, F R S 460
Art IX —Habit, Physiologically considered (A Lecture delivered at the Bristol Literary and Phil Institution) By J A Symonds, M D 461
Art X —Remarks on Hysteria, in connexion with Hydrophobia, and other Convulsive Affections By John Dalziel, M D ib
Art XI —Reports by Neil Arnott, M D, and Thomas Page, Esq C E on an Inquiry ordered by the Secretary of State relative to the Prevalence of Disease at Croydon, and to the Plan of Sewerage (Parliamentary Paper) 1853 462
Art XII —Small Pox and Vaccination A Report on the state of Small Pox and Vaccination in England and Wales and other countries (Parliamentary Paper, 3rd May, 1853) 463

Original Communications.

PAGE

ART I—The Blood—its Chemistry, Physiology, and Pathology By THOMAS WILLIAMS, M D Lond, Extra Licentiate of the Royal College of Physicians, formerly Demonstrator on Structural Anatomy at Guy's Hospital 46

ART II—Decennium Pathologicum, or, Contributions to the History of Chronic Disease, from the St George's Hospital Records of Fatal Cases during Ten Years By THOMAS K CHAMBERS, Physician to St Mary's Hospital 48

ART III—The Internal Surface of the Uterus after Delivery, its Analogies, &c By J MATTHEWS DUNCAN, A M, F R C P E, Lecturer on Midwifery, Edin, &c &c 50

ART IV—Historic Data &c, in reference to some points of Infantile Pathology By W HUGHES WILLSHIRE, M D Edinburgh, senior Physician to the Royal Infirmary for Children &c 51

ART V—On a Case in which the Sarcina Ventriculi, seu Sarcina Goodsirii was found in the fluid removed from the ventricles of the human brain By WILLIAM JENNER, M D, F R C P, Professor of Pathological Anatomy, University College, Physician to the Hospital for Sick Children, &c 53

Chronicle of Medical Science.

Annals of Micrology—Part II Pathological Micrology
- Histolysis, or Decay and Dissolution of Tissues 53
- Degenerations 53
- Abnormal Conditions of Blood ib
- Miscellaneous Lesions 537
- Tumours 54

Anatomy, Physiology, and Organic Chemistry
- The Determination of Urea By Professor LIEBIG 547
- On the Effect of Coffee By Dr ZOBEL 54
- The Effect of Tea By Dr BOCKER 54
- On the Constituents of the Flesh fluid of Frogs By Dr GROBE 55
- On the Action of Water By Dr FALCK ib

Pathology and Practice of Medicine
- On the Condition of the Grey Substance of the Brain after excessive Mental Exertion By Dr ALBERS 551
- On the Secretion of Sugar by the Liver, and the Modification of this by Disease By M VERNOIS ib
- On Intermittent Pneumonia By Dr CONSTANT 55

Surgery
- On the Reduction of Luxations of the Extremities, when complicated with Fracture By M RICHET 55
- On Traumatic Effusions of Serosity By M MOREL LAVALLEE ib
- Case of Hydatids of the Breast By M MALGAIGNE 554
- Contribution to Ophthalmoscopia. By Dr F CZERMAK 554

Midwifery &c
- Anteflexion of Uterus as a normal condition prior to Pregnancy By M BOULARD 55
- On the Effects of Menstruation on the Milk of Nurses By MM BECQUEREL and VERNOIS 55
- Reposition of the Prolapsed Funis By Dr FINCKE ib
- Stricture of the Fundus Uteri a cause of dangerous Hæmorrhage By Dr DIEZ 55

Therapeutical Record
- Aneurism—Asthma, Spasmodic—Cardialgia—Chloroform, Poisoning by—Cholera, Asiatic—Collodion—Creosote—Delirium Tremens—Epilepsy—Fever, Continued—Fever Intermittent—Furunculus—Gleet—Insanity—Leucorrhœa—Menorrhagia—Neuralgia—Ophthalmia Granular Lids—Otalgia—Pertussis—Phthisis—Porrigo—Rheumatism—Spina Bifida—Strabismus—Tænia—Tinea Favosa—Tumour, Erectile, of the Orbit—Ulcers—Urine, Incontinence of—Uterus, Prolapsus of—Worms 557—561

POSTSCRIPT 561
BOOKS RECEIVED FOR REVIEW 56
TITLE CONTENTS INDEX

PUBLIC LIBRARY

THE
BRITISH AND FOREIGN
MEDICO-CHIRURGICAL REVIEW.

OCTOBER, 1853

PART FIRST

Analytical and Critical Reviews

REVIEW I

1 *De Partibus Similaribus* By G FALLOPIUS (Date uncertain, before 1562)
2 *Theoria Generationis* By CASPAR FRIEDRICH WOLFF 1759
3 *Theorie von der Generation* By C F WOLFF 1764
4 *Theoria Generationis* By C F WOLFF Ed Nova. 1774
5 *Von der eigenthumlichen und wesentlichen Kraft der Vegetabilischen sowohl als auch der animalischen Substanz* By C F WOLFF 1789
6 *Entwickelungs Geschichte d Thiere.* By K E VON BAER. 1828
7 *Untersuchungen uber Phytogenesis* By SCHLEIDEN 1837
8 *Mikroskopische Untersuchungen* By SCHWANN 1838–9
9 *Ueber das Bindegewebe.* By REICHERT 1845
10 *Die Vegetabilische Zelle* By H VON MOHL 1850
The Vegetable Cell By H VON MOHL. Translated by A HENFREY, F R S 1852
11 *On the Mutual Relation of the Vital and Physical Forces* By W B CARPENTER, M D, F R S, &c ('Phil Trans') 1850
12 *Handbuch der Gewebelehre* By A KOLLIKER. 1852

IF we separate the elementary and essential facts of Life from all the various and complicated phenomena with which they are associated among the higher forms of living beings—if we examine those lowest and most rudimentary states of animal and vegetable existence, which are presented to us by the so-called 'unicellular' organisms, we find that the sole definable difference between a living thing and a mere formed morsel of some protein compound, fresh from the laboratory of the chemist, is, that while the protein compound undergoes no change which may not be traced to the immediate and direct operation of some new or varying external condition, the Navicula or the Gregarina passes through the most remarkable successions of form, of size, and of chemical composition, which are equally definite in their nature, and equally certain in the order of their occurrence, whatever, within certain limits, be the change in external conditions, or whatever pains may be taken to prevent any variation in them

Broadly, we may thus state the difference between the subjects of Physical and those of Biological Science the former, the stone, the gas, and the crystal, have an *inertia*, they tend to remain as they are, unless some external influence affect them The latter, animals and plants, on the other hand, are essentially characterized by the very opposite tendencies As Reichert well expresses it

> "All organic bodies, therefore, represent, in relation to one another, different and manifold states succeeding one another definitely upon a similar and homogeneous foundation, they form a common differential series, in which, *independently of external conditions*, a continual increase in the mutual differences and a diminution of the resemblances, occur" (p 12)

Linnæus seems to have wished to express his insight into this difference between living and dead matter, in his celebrated aphorism "Stones grow, plants grow and live," but so long as this "and live" was not analyzed into its true meaning, the phrase marked the difference, but failed to define it

Bichât recognised the independence of action of living beings in another way All things which surround living beings tend, he says, to destroy them, but they nevertheless follow out their own appointed course "La vie est l'ensemble des fonctions qui resistent à la mort"[*]

Now, this faculty of pursuing their own course, this inherent law of change, introduces, it will be observed, an element into the study of living beings, which has no analogue in the world of ordinary matter The latter frequently possesses structure, and may therefore be the legitimate subject of anatomy, but it undergoes no definite cyclical alterations,[†] and, therefore, it offers nothing which corresponds with what in living beings is called the History of Development, that branch of the investigation of structure which does not concern itself with the mere study of one state of a being—like anatomy—but examines into the manner in which the successive anatomical states are related to, and proceed out of, one another

---

[*] It is amusing to find M Comte, a mere bookman in these subjects devoting a long argument (Philosophie Positive, tom iii p 288) to a refutation (?) of what he calls the 'profonde irrationalité' of Bichât's definition As a specimen of the said refutation we may select the following passage "Si comme le supposait Bichat tout ce qui entoure les corps vivans tendait réellement à les detruire, leur existence serait par cela meme radicalement inintelligible; car, où pourraient-ils puiser la force nécessaire pour surmonter même temporairement un tel obstacle?" What a question for a positive philosopher! Does M Comte doubt his own power to get up from his easy chair because it is unquestionably true that the action of the whole globe 'tends" to retain him in his sitting posture and because he cannot tell whence he gets the force which enables him to rise?

[†] It is not easy to frame a definition of the differences between living and not living bodies which shall perfectly defy cavil That in the text—based on the inertia of not living bodies the internal activity of living bodies—marks the difference strongly but not unobjectionably, for it might be said that a nebula undergoing change would, by this definition be a living body and in the next place it might be urged how do we know that the activity of living bodies is not really the result of some external cause with which we are unacquainted? It might be said, that the apparent absence of change in external conditions is no more evidence that the vital phenomena are independent of some such causes than the continuous running of a stream when the dam is opened, independent of any further alteration of external conditions is evidence of spontaneity The action of the spermatozoon, e.g might be compared to the raising of the dam We have preferred above, however a vivid to an exact definition of our conception of life as likely to be more useful If we were to attempt an exact definition, it would be that a living being is a natural body which presents phenomena of growth of change of form and of chemical composition of a definite nature, and occurring in definite cycles of succession This definition will separate living beings from all other terrestrial bodies. It separates them from cosmical bodies (nebulæ &c) only by the nature of the phenomena which succeed one another; so true is it that the microcosm and the macrocosm are reflections of one another

A profound physiologist and thinker, a contemporary and worthy rival of Haller, has beautifully expressed the relation of anatomy, of physiology, and of development (which he calls Generation), in the following words

> "The relations between anatomy, the doctrine of generation, and physiology, are about these By anatomy we learn from observation the composition and structure of an organized body We, however, are unable to explain this composition and structure, we only know that they are thus, and further than this we know nothing But now, on the one hand, comes the doctrine of generation, in which that which we know from anatomy historically, is traced to its causes, on the other hand, we have physiology, in which the actions which an organized body is capable of producing are explained Physiology is related to anatomy exactly as the corollary to the theorem from which it is deduced, my theory (of generation) is related to anatomy as its demonstration to the theorem"*

And again we find the relation of development to anatomy admirably and epigrammatically expressed in the 'Theoria Generationis' of the same writer Development is, he says, "anatomia rationalis"

It has been said, and without doubt with profound truth, that the study of the structure of living beings originated in the wonder excited by their actions But though this may, nay must, have been the case at first, and though the curiosity of man has for three centuries past directed itself, with almost equal impartiality, to physiological, anatomical, and developmental inquiries, still it is clear, that if the above account of the correlation of these branches of science be correct, their logical connexion, and the order, therefore, in which they must eventually arrive at perfection, is precisely the reverse The striking and mysterious character of many of the functions may have led to the study of structure, but assuredly the understanding of the former presupposes a thorough knowledge of the latter

It is conceivable that structure might be thoroughly made out without the least acquaintance with function, just as the ancient anatomists were well acquainted with the construction of the muscular system, and yet had no suspicion of its being the motor apparatus, and as at the present day we know full well the structure of the "vascular glands," though we can but guess their purpose, but it is quite impossible to attain to a complete knowledge of function without a thorough anatomical analysis The action of the whole of any organ depends upon and is, that of the sum of its parts, it is, mechanically speaking, their resultant, so that until the nature and the precise modes of operation of all these parts have been made out, we can have no security that any law propounded concerning the functions of the whole, is other than a mere empirical generalization, liable to be interfered with at any moment, by the properties of some of the elementary parts with which we are unacquainted Thus, up to within a few years ago, contractility was affirmed to be a general property of the cellular tissue of the skin, and this would have remained as an ascertained law, had not Kolliker shown, by the discovery of the extensive distribution of muscular fibre in it (that is, by pushing anatomical analysis a step further than it had previously been carried), that the supposed law was but an empirical generalization, and that the property of contractility, supposed to be inherent in the ordinary connective tissue of the skin, was, in fact, deducible from the presence of a totally different structure

---

* Wolff Theorie von der Generation, p 12

So again, Haller and his followers quoted the contraction of the heart, when removed from the body, as evidence of the innate contractility of muscle, apart from all nervous influence This *vis insita* may exist or it may not, but further anatomical investigation has at any rate destroyed the force of the argument, by demonstrating the existence of nervous ganglia within the substance of the organ

But enough of illustration of what must be sufficiently plain to any one who will reflect upon the subject, namely, that however much might be done towards the establishment of broad physiological truths, while the knowledge of structure was in a rough and imperfect state, still an exhaustive study of structure is absolutely necessary, before any successful attempt can be made to establish the true laws of function, or to build the science of physiology upon an exact foundation

Herein lies, consciously or unconsciously to their authors—for the man of genius is such, in virtue of having true and just tendencies and impulses, of which he often can give himself no logical account—the secret of the repeated attempts which have been made from the time of the very fathers of biology, to found what we now call the doctrine of general anatomy or histology, which is, in other words, the exhaustive anatomical analysis of organized bodies That animals and plants, complex as they may appear, are yet composed of comparatively few elementary parts, frequently repeated, had been noticed by the profound intellect of Aristotle, and Fallopius tells us, that Galen had attained to still more clear and definite conceptions, with regard to these "partes similares' or "simplices "*

> "Galenus per simplices partes eas intelligit quæ non constant ex dissimilibus substantiis, in quas resolvitur corpus humanum, nec ultra datur progressio et istæ partes dicuntur simplices quia cum ad hoc ventum fuerit in resolutione corporis humani, non amplius progredi possumus" (p 103)

Such, indeed, must be the definition of elementary parts at all periods of science—they are ultimate, because we can go no further, though it is of course a very different matter whether we are stopped by the imperfection of our instruments of analysis, as these older observers were, or by having really arrived at parts no longer analyzable

The celebrated professor of Modena, whose words we have just cited, was one of the first of those who carried the light shed by the revival of letters into the region of medicine and its allied sciences, and his work 'De Partibus Similaribus,' from which they are taken, must excite the admiration of every modern reader, not merely by the critical acumen and original genius which it displays, but by the scientific and absolutely accurate manner, in which the whole subject of general anatomy is handled

The classes of "partes similares," or tissues, of which he treats, are bone, cartilage, fat, flesh, nerve, ligament, tendon, membrane, vein, artery, nails, hairs, and skin, and he examines and details under each head the minute structure, so far as it was accessible to his means of investigation, the chemical and physical properties (expressed, of course, in the language of the day), and even the peculiarities manifested by the diseased state. Nor is he at all wanting in what has been considered, and justly, to be Bichât's great merit—an essentially *positive* method of studying the tissues, inasmuch as he particularly insists on the necessity of investigating the properties of each tissue for itself, and of avoiding all hypothetical speculation; in fact, with the quaint plainness of the age, he does not hesitate to insinuate, that Averrhoes must have been "ebrius" when he discoursed touching "spiritus qui insensibiles sunt."

---

* Terms by no means always convertible, but which may for the present be taken to be so

The vitality of each tissue, independently of every influence save the general conditions of nutrition, is maintained by Fallopius, not as a mere speculation, but on sound embryological grounds. How can the liver, he asks, be the sole source and prime mover of all vital organization, as some have maintained, when, in the development of the chick, we see other organs appear before it? All that the liver and the vessels can do, is, to modify the supplies, by affecting the "restitutiones spirituum ac nutrimentis" (p. 98), the "partes similares" themselves having a "regimen insitum," or, as in our day it would be called, "vital force" of their own; and he quotes, as expressing his own views, the following remarkable passage from Actuarius:

> "Quod partes naturales agunt propria forma ac cum instrumento quod dicitur spiritus animalis: nam hoc instrumento per propriam formam attrahunt, concoquunt et expellunt, et hic spiritus est immediatum instrumentum vis naturalis, et hic spiritus dicit Actuarius, *originem ducit unâ cum formâ ipsius particulæ, et ex eâdem materiâ eodemque tempore fit*."

Substitute here for the indefinite "particulæ" definite vesicular particles or cells, and for "spiritus animalis" the modern terms of equivalent meaning or no meaning—vital-force or cell-force, and this passage would serve very well as a concise expression of the "cell theory," such as may be found in many a hand-book of the day. So far, and no further, have three centuries brought us!

In fact, it must be confessed, that these old writers were fully possessed (more so, in truth, than many of their successors) with the two fundamental notions of structural and physiological biology; the first, that living beings may be resolved anatomically, into a comparatively small number of simple structural elements; the second, that these elementary parts possess vital properties, which depend for their manifestation only upon the existence of certain general conditions (supply of proper nutriment, &c.), and are independent of all direct influence from other parts.

But it would seem, that Truth must pass through more than one Avatar, before she can attain a firm hold upon the mind even of men of science—and at the end of the eighteenth century it required all the genius of Bichât to sift the wheat from the chaff, amongst the great mass of facts which the observation of the past ages had accumulated—and strengthening whatever place was weakest by new investigations—to establish these very two propositions, upon a broad and henceforward firm foundation. Great as was the service which Bichât rendered in this way to biology—and wide as the difference between the treatise 'De Partibus Similaribus' and the 'Anatomie Generale' may be—still the one is the intellectual progeny of the other, and exhibits neither alteration nor improvement in the method pursued.

In the meanwhile, however, an aid to investigation had arisen, by the means of which this method could be pushed to its uttermost limits—we refer to the invention of the microscope The influence of this mighty instrument of research upon biology, can only be compared to that of the galvanic battery, in the hands of Davy, upon chemistry It has enabled *proximate* analysis to become *ultimate* Without the microscope the ultimate histological elements were, as we have seen, defined *negatively*, as parts in which any further structural difference was too small to be detected The microscope, on the other hand, enables us to define the tissues *positively* —to say, a given tissue has such a structure, and magnify it as you will, it will present no further differences

The amount of such positive information as to the ultimate structure of the tissues, collected by Leuwenhoek, Malpighi, and their successors, between the middle of the seventeenth and the fourth decade of the present century, was very great, and in fact the most important and characteristic features presented by the histological element of plants and animals may be said to have been well made out, at the time of the appearance of the celebrated treatises by Schleiden and Schwann, cited at the head of this article, and these writers, therefore, added but little to the body of knowledge in this direction It is most unquestionable, however, that the biological sciences, and more especially histology, received a wonderful stimulus at their hands Whatever cavillers may say, it is certain that histology before 1838 and histology since then, are two different sciences—in scope, in purpose, and in dignity—and the eminent men to whom we allude, may safely answer all detraction by a proud "*circumspice*"

But wherein does the real value of their work lie? We think this question may be readily enough answered by those who admit the force of what has been said in our opening paragraphs—who acknowledge that mere anatomy does not exhaust the structure of living beings—and that before histology can be said to be complete, we must have a histological *development*, as well as a histological *anatomy* Leuwenhoek and the majority of his successors had enough to do in making out the "*historicam cognitionem*," the simple anatomy of the tissues, it tasked all their powers to arrive at a clear statement of the "*theorem*," while it is the great merit of Schleiden and Schwann, that they sought to arrive at an "*anatomia rationalis*," and to furnish the "demonstration of the theorem" The old method of investigation had been carried as far as it would go, and they applied the only other which remained, and made it familiar to the general mind Turn to any of Schleiden's works, and we find the logical acuteness, and the vituperative sarcasm, which he wields with equal force, employed in urging the study of development as the one thing needful for scientific botany And Schwann's entire essay testifies to what he expressly tells the reader, that his investigations are distinguished from all others by being based upon the study of development Let one citation suffice —"The theory of the present investigation was, therefore, to show that there exists a common principle of development for all the elementary parts of the organism" (Schwann, pp 193—196)

Intending as we do to venture upon a critical examination of the absolute value of Schleiden's and Schwann's contributions to biological science, which may lead us to conclusions not ordinarily admitted, we have been particularly desirous to estimate fairly the position which they occupy in its history, and the influence which their labours have had upon its progress—which is a widely different matter—for, in attempting to weigh the labours of others, we should be in danger of committing great injustice, if we did not carefully bear in mind, that, paradoxical as it may seem, the value of a theory and its truth, are by no means commensurate. In so complex a science as that which relates to living beings, accurate and diligent empirical observation, though the best of things as far as it goes, will not take us very far; and the mere accumulation of facts without generalization and classification is as great an error intellectually, as, hygienically, would be the attempt to strengthen by accumulating nourishment without due attention to the primæ viæ, the result in each case being chiefly giddiness and confusion in the head.

In biology, as in all the more complicated branches of inquiry, progress can only be made by a careful combination of the deductive method with the inductive, and by bringing the powerful aid of the imagination, kept of course in due and rigid subordination, to assist the faculties of observation and reasoning; and there are periods in the history of every science when a false hypothesis is not only better than none at all, but is a necessary forerunner of, and preparation for, the true one. As Schwann himself well expresses it:

> "An hypothesis is never hurtful, so long as one bears in mind the amount of its probability, and the grounds upon which it is formed. It is not only advantageous, but necessary to science, that when a certain cycle of phenomena have been ascertained by observation, some provisional explanation should be devised as closely as possible in accordance with them; even though there be a risk of upsetting this explanation by further investigation; for it is only in this way that one can rationally be led to new discoveries, which may either confirm or refute it." (p. 221.)

The value of an hypothesis may in fact be said to be twofold—to the original investigator, its worth consists more in what it suggests than in what it teaches; let it be enunciated with perspicuity, so that its logical consequences may be clearly deduced, and made the base of definite questions to nature—questions to which she must answer yes or no—and of its absolute truth or falsehood, he recks little: for the mass of men, again, who can afford no time for original research, and for the worker himself, so far as respects subjects with which he is not immediately occupied, some system of artificial memory is absolutely necessary. This want is supplied by some "appropriate conception" which, as Dr. Whewell would say, "colligates" the facts—ties them up in bundles ready to hand—by some hypothesis, in short. Doubtless the truer a theory is,—the more "appropriate" the colligating conception,—the better will it serve its mnemonic purpose, but its absolute truth is neither necessary to its usefulness, nor indeed in any way cognizable by the human faculties. Now it appears to us that Schwann and Schleiden have performed precisely this service to the biological sciences. At a time when the researches of innumerable guideless investigators, called into existence by the tempting facilities offered by the improvement of microscopes, threatened to swamp science in minutiæ, and to render the noble calling of the physiologist identical with that of the 'putter-up' of preparations, they stepped forward with the cell-theory as a colligation of the facts. To the investigator, they afforded a clear basis and starting-point for his inquiries, for the student, they grouped together immense masses of details in a clear and perspicuous manner. Let us not be ungrateful for what they brought. If not absolutely true, it was the truest thing that had been done in biology for half a century.

But who seeks for absolute truth? Flattering as they were to our vanity, we fear it must be confessed that the days of the high *à priori* road are over. Men of science have given up the attempt to soar eagle-like to some point amidst the clouds, whence the absolute relations of things could be securely viewed, and at present, their more useful, if more ignoble course, may rather be compared to that of the flocks of sparrows in autumn, which one sees continually halting, yet always advancing—flying from tree to tree, noisily jubilating in each, as if that were assuredly the final resting-place and secure haven of sparrows, and yet as certainly taking their departure after awhile, in search of new acquisitions. We must build our theories, in these days, as we do our houses: giving up all attempt at Cyclopean architecture, let us bethink ourselves rather of the convenience of our successors, who will assuredly alter, and perhaps pull them down, to suit the needs of their own age, and if we seek their gratitude, let us strive not so much to knit our materials firmly together (which will only give them more trouble and yield us less thanks), as to see that they are separately sound and convertible. This much digression has seemed necessary, by way of securing ourselves from any suspicion of a desire to under-estimate the historical value of Schleiden and Schwann's researches, in the course of an attempt to show that they are based upon errors in anatomy, and lead to errors in physiology.

Again, with regard to that value, we have a few words to say in a merely historical point of view. The sketch we have given of the progress of general anatomy, we believe, omits mention of no ordinarily recognised epoch, nor fails to indicate the acknowledged order of the successive introduction of those great leading ideas with which we are at present concerned.* In their own belief, and in that of their contemporaries, Schleiden and Schwann have not only worked out developmental histology, but originated it, and the latter, in his reclamation against Valentin (loc. cit., pp. 260, 261), defends his claim to be considered the originator of the idea that "a common principle governs the development of the elementary parts of all organisms." Now, we fully recognise the originality of these writers, we believe that they deserve all the credit which can attach to a noble plan carried out with no small success, and we further remember that the majority always sympathizes with the cry, "Pereant qui ante nos, nostra," &c., but, as we have said, truth often has more than one Avatar, and whatever the forgetfulness of men, history should be just, and not allow those who had the misfortune to be before their time, to pass for that reason into oblivion.

Such was the position into which his great genius forced Caspar Friedrich Wolff—such the fate with which he has met. The manuals of physiology tell us that he was the founder of the doctrine of epigenesis—

---

* Compare Kolliker's Handbuch, Introduction; or Sprengel's Geschichte der Arzneikunde

a doctrine which, in the present day, seems so plain and obvious, that we do not give him much credit for it, forgetting that he had to struggle against the authority of Malpighi and of Haller, and the attacks of Bonnet, influence and authority so great, that though every reader of the 'Theoria Generationis' must see that Wolff triumphantly establishes his position, yet, seventy years afterwards, we find even Cuvier* still accrediting the doctrine of his opponents

It is less generally (we might say hardly at all) known that Wolff demonstrated, by numerous observations on development, the doctrine of the metamorphosis of plants, when Gothe, to whom it is commonly ascribed, was not quite ten years old,† but it seems to have been wholly forgotten that he endeavoured to work out, upon the basis of the strict study of histological and morphological development, that "identity of structure of plants and animals" which is the thesis defended by Schwann Had Wolff's teaching been founded upon one of those clever guesses upon which an able man will often build up a plausible hypothesis, we should have thought it quite unnecessary to make even historical reference to him, but the most cursory examination of the 'Theoria Generationis,' or of the more popular and discursive exposition of his views in the 'Theorie von d Generation,' is enough to dispel any such notion The passage we have already quoted is sufficient to show how just and accurate Wolff's ideas upon the importance of the study of development, as a method, were, and the whole of his work is the laborious application of that method The parts of the calyx, of the corolla, and of the pericarp, are for him "modified leaves," not because certain observed modifications had suggested that they might be so considered—which is the whole gist of Gothe's subsequent argument—but because he had carefully traced back their development, and had found that they all proceeded from the same original form The homology of the wing of the chick with its leg is placed by Wolff on precisely the same basis—the only one, be it observed, on which any homology can ultimately rest, and following out the argument to its legitimate conclusion, he shows that the appendicular organs of plants and animals are developed after the same fashion The limbs of animals, he says, are developed in the same manner from the body of the embryo, as the leaf from the stem, or the lamina of the leaf from its mid rib Ordinary four-footed animals are like pinnatifid leaves, while "the bat is a perfect leaf—a startling statement, but, as I have shown, the analogy is not chimerical, for the *mode of origin of the two is the same*"‡

Wolff's doctrine concerning histological development is shortly this § Every organ, he says, is composed at first of a little mass of clear, viscous, nutritive fluid, which possesses no organization of any kind, but is at most composed of globules In this semi-fluid mass, cavities (*Bläschen, Zellen*) are now developed, these, if they remain rounded or polygonal, become the subsequent cells—if they elongate, the vessels, and the process

---

[*] Histoire des Sciences Naturelles

[†] The world always too happy to join in toadying the rich and taking away the "one ewe lamb from the poor persists in ascribing the theory of the metamorphosis of plants to Gothe, in spite of the great poet himself (see Gothe s Werke, Cotta 1840 B 36 p 105 Entdeckung eines trefflichen Vorarbeiters") who not only acknowledges his own obligations to Wolff but speaks with just wonder and admiration of the Theoria Generationis,' the work of a young man of six and twenty

[‡] Theorie von der Generation § 61

[§] Theoria Generationis and Von der eigenthümlichen Kraft p 48 is identically the same, whether it is examined in the vegetating point of a plant, or in the young budding organs of an animal Both cells and vessels may subsequently be thickened, by deposits from the "solidescible" nutritive fluid In the plant, the cells at first communicate, but subsequently become separated from one another, in the animal, they always remain in communication In each case, they are mere cavities, and not independent entities, organization is not effected by them, but they are the visible results of the action of the organizing power inherent in the living mass, or what Wolff calls the *vis essentialis* For him, however, this "vis essentialis" is no mythical *archeus*, but simply a convenient name for two facts which he takes a great deal of trouble to demonstrate, the first, the existence in living tissues (before any passages are developed in them) of currents of the nutritious fluid determined to particular parts by some power which is independent of all external influence, and the second, the peculiar changes of form and composition, which take place in the same manner

Now there is really no very great difference between these views of the mode of development of the tissues, and those of Schleiden and Schwann The "solidescible nutritive fluid" of Wolff is the "cytoblastema" of Schleiden and Schwann, with the exception of the supposed relation of the nucleus to the development of the cell (which, as we shall see, is incorrect) Wolff's description of the latter process is nearly that of Schleiden, Wolff maintains that the "vessels" of plants are the result of the greater activity of the nutritive currents in particular directions, and so does Schleiden *

Examining his statements closely, we notice, indeed, that his imperfect means of investigation led Wolff into two important errors—that of supposing the cells of plants to communicate in their youngest state, and thence deducing a false analogy with the areolar tissue of animals, and that of supposing that animal and vegetable tissues are always, in their very youngest state, absolutely structureless However, as we shall see subsequently, Wolff is by no means singular in having started with grave anatomical mistakes, and we cannot perceive that in his case these errors, one of which, at any rate, Schleiden shares with him, vitiate those other and more important parts of his views, to which we are about to refer

We have said, in fact, that not merely speculatively, but by observation, Wolff established a theory of the development of the vegetable tissues very similar to that of Schleiden, and that "identity of structure as shown by their development," between plants and animals, to prove which, was the purpose of the microscopical investigations of Schwann

---

* It is very curious to find even Schwann s definition of cell development as the 'crystallization of a permeable body anticipated by Wolff Von d eigenthumlichen Kraft, &c p 63 so from the case of many other attractions especially of crystallization which among all known phenomena, comes nearest to vegetation that without the second property—viz that by means of which the mutually attractive substances interpenetrate and mingle with one another—the attractive force although it should possess the first property yet could as little effect nutrition The particles of a salt dissolved in water attract only particles of salt—i e homogeneous substances, and repel all heterogeneous matters; for we get pure (crystals of the) salts But out of this attraction comes nothing that can be compared with nutrition, for although the whole mass of substance is increased by degrees yet crystals once formed remain as they are and are not increased in their substance and nothing less than the formation of new organization and continual change of figure accompanies this increase Crystals once formed attract the saline particles only to their outer surface upon which the attracted parts are deposited They do not attract these particles into their substance If the saline particles, on the other hand penetrated the crystals and increased their substance homogeneously in all parts this process would be indistinguishable from nutrition, and would consequently be a true nutrition" Compare Schwann, pp 239—257

But he did much more than this In the 'Theoria Generationis,' and in the essay on the vital forces published thirty years afterwards, Wolff developed some very remarkable views on the relation of life to organization —of the vital processes to the organic elements—in which he diverges very widely from all who preceded, and from most who have followed him, most of all from Schleiden and Schwann We may best exhibit the bearing of these views by contrasting them with those of the latter writers

Schleiden and Schwann teach implicitly that the primary histological elements (cells) are independent, anatomically and physiologically, that they stand in the relation of *causes* or *centres*, to organization and the "organizing force," and that the whole organism is the result of the union and combined action of these primarily separate elements Wolff, on the other hand, asserts that the primary histological elements (cells too, but not always defined in the same way) are not either anatomically or physiologically independent, that they stand in the relation of *effects* to the organizing or vital force (vis essentialis), and that the organism results from the "differentiation" of a primarily homogeneous whole into these parts Such a doctrine is, in fact, a most obvious and almost a necessary development of the doctrine of epigenesis in general To one who had worked out the conclusion, that the most complex, grosser, animal or vegetable organizations, arise from a semi-fluid and homogeneous mass, by the continual and successive establishment of differences in it, it would be only natural to suppose that the method of nature, in that finer organization which we call histological, was the same, and that as the organ is developed by the differentiation of cells, so the cells are the result of the differentiation of inorganic matter If the organism be not constituted by the coalescence of its organs and tissues in consequence of their peculiar forces, but if, on the other hand, the organism exists before its organs and tissues, and evolves them from itself,—is it not probable that the organs and tissues also, are not produced by the coalescence of the cells of which they are composed, in consequence of *their* peculiar forces, but, contrariwise, that the cells are a product of the differentiation of something which existed before them?

For Schwann the organism is a beehive, its actions and forces resulting from the separate but harmonious action of all its parts (compare Schwann, l c, p 229) For Wolff it is a mosaic, every portion of which expresses only the conditions under which the formative power acted and the tendencies by which it was guided

We have said above, not without a full consciousness of the responsibility of the assertion, that we believe the cell theory of Schleiden and Schwann to be based upon erroneous conceptions of structure, and to lead to errors in physiology, and we beg now to offer some evidence in favour of these views We need not stop to prove, what must be familiar to every one who is acquainted with Schwann's work, that in making his comparison of animal with vegetable structures, he rests wholly upon Schleiden's statements concerning the development, and upon the commonly prevalent views with respect to the anatomy, of the latter

It is clear, then, that however logically consequent Schwann's work may be in itself, its truth and the justice of its nomenclature will depend upon that of these latter views and statements Schwann took these for granted, and if they were untrue he has been trusting to a rotten reed. Such, we fear, has indeed been the case Schwann's botanical data were

1 The prevalent notion of the anatomical independence of the vegetable cell, considered as a separate entity
2 The prevalent conception of the structure of the vegetable cell
3 The doctrine of the mode of its development

Each of these, as assumed by Schwann, and as taught by Schleiden, has since, we shall endeavour to show, been proved to be erroneous We will take them *seriatim*

1 The first observer who, aided by the microscope, turned his attention to the structure of plants, was the versatile Hooke, and, as might be expected, the most noticeable thing to his mind was the existence of the innumerable cavities or "cells" scattered through their substance Malpighi, the first proper botanical histologist, found that the walls of these vesicles were separable, that they could be isolated from one another, and therefore, doubtless urged more by the obvious convenience of the phraseology, than by any philosophical consideration upon the subject, he gave each the definite name of "utriculus," and regarded it as an independent entity Of course it was a natural consequence that the plant should be regarded as constituted by the *union and coalescence* of a great number of these entities

Grew, who, if all scandal be true, is so much indebted to Malpighi, did not appropriate this view among other things, on the other hand, he compared the utricles to the cavities in the foam of beer, and subsequently Wolff propounded the idea, that the cells were cavities in a homogeneous substance, as we have mentioned above In modern times, the most important defender of this mode of regarding the matter has been Mirbel, who (escaping the error of Wolff, that the cavities of the cells communicate) endeavoured to demonstrate its truth, by tracing the formation of the cambium, but, at the time when Schwann wrote, it must be considered to have been wholly discredited, the opposite view having one of its strongest supporters in the caustic Schleiden himself—as, indeed, would necessarily be the case, from the tendency of his researches upon phytogenesis As we shall see below, however, Schleiden was quite wrong in his ideas of cell development—and we have therefore merely to consider the purely anatomical arguments for the independence of the cell Now these amount, however various their disguise, to nothing more than this—that, by certain chemical or mechanical means, a plant may be broken up into vesicles corresponding with the cavities which previously existed in it of course no one denies this fact, but of what value is it? Is the fact, that a rhombohedron of calcareous spar breaks up, if pounded, into minute rhombohedrons, any evidence that those minuter ones were once independent, and formed the larger by their coalescence? Is the circumstance that wood itself tears up into fibres, any evidence that it was formed by the coalescence of fibres? Assuredly not, for every hand-book will tell us that these fibres are the result of a metamorphosis of quite different parts Is it not perfectly clear, that the behaviour of a body under mechanical or chemical influences, is simply an evidence of the disposition of the lines of greatest cohesion or affinity among its particles *at* *the time being*, and bears not in the slightest degree upon the question as to what these lines indicate; whether they are the remains of an ancient separation among heterogeneous parts, or the expression of a recent separation which has arisen in a homogeneous whole. So that, if the walls of the cells were really, as distinct from one another as is commonly supposed, it would be no argument for their vital independence: but they are not so. Von Mohl has shown that, in the great majority of cases, the assumption of the existence of a so-called intercellular substance, depends simply on imperfect chemical investigation; that there exists no real line of demarcation between one cell and another, and that wherever cells have been separated, whether mechanically or chemically, there is evidence that the continuous cellulose substance has been torn or in some way destroyed. In young tissues—such, for instance, as the cambium, or the base of a leaf, we have been quite unable to detect the least evidence of the existence of any line of demarcation between the cells; the cellulose substance forms a partition between cavity and cavity, which becomes evenly blue throughout by the action of sulphuric acid and iodine, and which certainly, even under the highest powers, exhibits no symptom of any optical difference; so that, in this state, vegetable tissue answers pretty closely to Wolff's idea. It is a homogeneous cellulose-yielding, transparent substance, containing cavities, in which lie peculiar vesicular bodies, into whose composition much nitrogen enters. It will be found a great aid if in the present confused state of terminology the reader will accept two new denominations for these elementary parts, which express nothing but their mutual relation. To the former, and to everything which answers to it, we shall throughout the present article give the name of *Periplast*, or periplastic substance; to the latter, that of *Endoplast*. So far, then, from the utricles or cells in the plant being anatomically distinct, we regard it as quite certain, that that portion which corresponds with the periplast, forms a continuous whole through the entire plant.

2. In 1837-8, each utricle of the plant was considered to have the following composition. In the first place, there was the cellulose cell-wall, or the portion of periplast answering to any particular endoplast; secondly, there were the cell-contents, a substance of not very defined nature, which occupied the cavity of the cell; and thirdly, there was the *nucleus*, a body to whose occurrence attention was first drawn, as is well known, by our own illustrious botanist, Robert Brown. He, however, cautiously remarked only its very general occurrence, without pretending to draw any inference from the fact; while Schleiden made the belief in its existence in all young tissues, the first article of the faith botanical. This is, however, most certainly incorrect; there is no trace of a nucleus in many Algæ, such as Hydrodictyon, Vaucheria,* Caulerpa; in the leaf of Sphagnum, nor in young germinating Ferns.†

Whatever opinion may be entertained upon this head, there is one point quite certain—the enumeration of the elements of the vegetable-cell given above is incomplete; there being one, and that the most important, which is omitted. We refer to the *primordial utricle*, which was only discovered by Von Mohl in 1844. This is a nitrogenous membrane, which always lies in close contact with the periplast, and forms, in fact, an included vesicle, within which the "contents" and the nucleus lie. Instead, therefore, of the endoplast consisting merely of contents and a nucleus, it is a vesicle containing the two latter, when they exist at all, and they are of subordinate importance, for while, as we have seen, a nucleus and formed contents may be absent in young or even fully formed tissues, the primordial utricle is invariably present in the young structures, and often persists until they have attained their full size Since, then, the functions of the vegetable "cell" can be effectually carried on by the primordial utricle alone, since the "nucleus" has precisely the same chemical composition as the primordial utricle, and since, in some cases of cell-division, new nuclei are seen to arise in the substance of the endoplast, by a mere process of chemical and morphological differentiation (Von Mohl, l c, p 52), it follows, we think, that the primordial utricle must be regarded as the essential part of the endoplast—the protoplasm and nucleus being simply its subordinate, and, we had almost said, *accidental* anatomical modifications

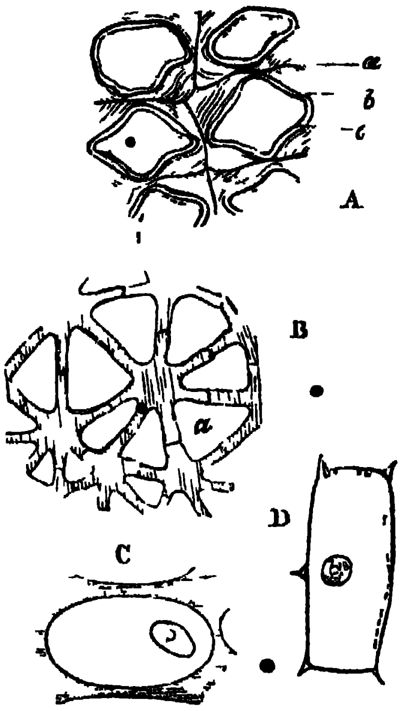
---

* On Alex. Braun's authority, Ueber Verjungung, &c., p. 186.

† Henfrey, Linn. Trans. 1853.

3 Finally, with respect to Schleiden's observations upon the mode of cell-development, according to which in all cases the new production of vegetable-cells takes place by the development of nuclei, round which the cell-membrane is deposited, subsequently expanding and becoming separated from the nucleus, so as to form a complete cell, we need only say, that they have been long since set aside by the common consent of all observers, in Von Mohl's words (p 59) "The whole of this account of the relation of the nucleus to the cell membrane is incorrect" The fact is, that in by far the greater proportion of cases, new cell-development occurs by the division of the previous endoplasts, and the growth or deposition round them and between them, of fresh periplastic substance The extent of this process of division will be understood, if we remember that all observers now agree in its being the method by which "cell-development" always occurs, except in the embryo-sac of the Phanerogamia, the sporangia of Lichens and of some Algæ and Fungi The so called free cell-development of the latter, however, by no means takes place in accordance with Schleiden's views, but by the development of a cellulose membrane (periplast) around a mass of nitrogenous substance (endoplast), which may or may not contain a nucleus, subsequently increasing, *pari passu*, with the periplast And it is well worthy of consideration, how far the process deserves any distinction, except in degree, from ordinary cell-division, since the new endoplast is only one portion of that of the parent cell, set aside for the purpose of fresh development, while the rest undergoes no corresponding change However this may be, it may be regarded as quite certain, that, leaving out of view the immediate results of sexual reproduction, the whole of the "cells,"—the entirety of the periplasts and endoplasts—of which a plant, whether it be a moss or an oak, are composed, never are independent of one another, and never have been so, at any period of their existence, but that, while the original endoplast of the embryo-cell, from which the plant sprung, has grown and divided into all the endoplasts of the adult, the original periplast has grown at a corresponding rate, and has formed one continuous and connected envelop from the very first The ground of his comparison, therefore, is cut away from under Schwann's feet, every statement of Schleiden's on which he relied turning out to be erroneous—as we shall see if we turn to his original comparison of cartilage with a vegetable tissue.(pp 9—17)

Fig 1



A Collenchyma cells of Beta vulgaris B Stellate tissue of the pith of the rush C a cartilage cell with its corpuscle to compare with D a vegetable cell with its nucleus the primordial utricle in the latter being indicated only by a dotted line

Schwann, finding in cartilage, cavities with more or less distinct walls, in each of which lay a corpuscle, singularly resembling the nucleus of the vegetable-cell, finding also that the cell-wall was close to this corpuscle in the younger parts, more distant from it in the older (p 24), naturally concluded that he had here, in the animal world, an exact confirmation of Schleiden's supposed discoveries, and of course gave to the corpuscle of cartilage the name of "cytoblast," or "nucleus," as indicating its homology with the structure of that name, in the plant

The primordial utricle was, as we have said, not then discovered in the latter, and of course Schwann was not led to look for anything corresponding to it Indeed, had he done so, his search would have been unsuccessful, for the young and unaltered cartilage cavity contains the corpuscle, and nothing else The circumstance, therefore, which Schwann considered to demonstrate the identity of structure of plants and animals—i e, the correspondence of the cartilage-corpuscle with the nucleus of the vegetable cell, and of the chondrin-wall with the cellulose-wall, would, if it were really the case, be the widest possible ground of distinction between the two, for it would leave the most important element of the latter, the "primordial utricle," without any homologue in the animal, and totally unaccounted for

It is precisely the neglect of this important change in the whole subject, effected by the discovery of Von Mohl, which has, we think, led to the confusion which prevails at present, not only in the comparative, but in the absolute nomenclature of animal histology Animal physiologists go on using Schwann's nomenclature, forgetting that the whole doctrine of the vegetable-cell, from which he drew that nomenclature, has been completely upset, and at present, beyond the mere fact of a common vesicularity at one period of their existence, one would be led, on opening successively two works on animal and vegetable structure, rather to predicate their total discrepancy, than any uniformity between them

Now does this discrepancy lie in the facts, or in our names of them? To decide this question, it seemed to us that the only plan was to follow Schwann's steps, and to compare cartilage with a vegetable tissue—for he has shown logically and conclusively enough, that whatever is true of the corpuscle of cartilage, to which he gives the name of "nucleus," is true also of all those corpuscles in the other tissues, to which he gives the same name

Let us compare, then, some young vegetable tissue, say that of the base of a Sphagnum leaf (fig 2 A), which is in many respects very convenient for examination, with that of young cartilage (fig 3 A), the identity of structure is such, that it would be difficult, without the aid of chemical re-agents, to distinguish one from the other in each, we see highly nitrogenous, more or less vesicular endoplasts imbedded in a homogeneous transparent substance, whose cavities they wholly fill If we trace the further development, we find that in the Sphagnum leaf the endoplasts and their cavities rapidly increase in size (fig 2 B), the former becoming, in certain localities of the leaf, regular primordial utricles without any nucleus, and growing in exact proportion to the cavities in the periplast (*b*), while in other directions, having attained a certain size they cease to grow, and rapidly disappear, leaving the periplastic cavity empty (*a*) In cartilage precisely the same thing occurs The endoplasts increase in size for awhile, and then stop, while the periplastic cavities continue to increase, and thus we have eventually a cartilage-cavity with its corpuscle In old cartilage the latter frequently disappears, or is converted into fat. We have here purposely selected, in both the animal and the plant, simple cases, in which the endoplast becomes a primordial utricle, without any nucleus Had we selected the cambium of a phœnogamous plant, it would have been merely necessary to add, that as the endoplast grew, a nucleus appeared in its interior, and in ossifying cartilage, near the ossifying surface, we have repeatedly seen endoplasts such as those described above, some of which contained definite "nuclei," while those in their immediate neighbourhood possessed none.

In the case of cartilage, then (and it is a conclusion at which Leidy and Remak have already arrived), we hold it to be proved, that the corpuscle does not correspond with the nucleus of the plant, as Schwann supposed, but with the primordial utricle, contents and nucleus, or, in other words, that the "nucleus" of cartilage, is the equivalent of the "primordial utricle" of the plant—that they are both endoplasts. It follows, hence, that the chondrin-wall of the cartilage, is the homologue of the cellulose wall of the plant, and that they both represent the periplastic element The phenomena of growth and multiplication exhibited by these corresponding elements are perfectly similar The process of cell-division, as it is called, is identical in each case. In the plant, the primordial utricles divide, separate, and the cellulose substance grows in between the two In young cartilage the same thing occurs, the corpuscles divide, separate, and the chondrin substance eventually forms a wall of separation between the two There is neither endogenous development nor new formation in either case The endoplasts grow and divide, the periplast grows so as to surround the endoplasts completely, and, except so far as its tendency is, to fill up the space left by their separation, there is no evidence that its growth is in any way affected by them, still less, that it is, as is often assumed, deposited by them We are led, then, to the conclusion, that though Schwann's great principle of the identity of structure of plants and animals is perfectly correct, his exposition of it is incorrect, inasmuch as the corpuscle of cartilage (his "nucleus," whence he reasoned to the other "nuclei,") answers not to the "nucleus," but to the "primordial utricle" of the plant, since the mode of development of new "cells," though identical in each case, is different from what Schleiden stated, and

Schwann believed; and finally since, for the notion of the anatomical independence of the cells, we must substitute that of the unity and continuity of the periplastic substance in each case.

Intimately connected with these structural errors, as we cannot but think them, are Schwann's views of the nature and powers of the "cell," and those subsequently developed (principally by Kolliker) with respect to the action of the nuclei as "centres of force." Led apparently by his views of its anatomical independence, Schwann maintains, as a general proposition, that the cell as such possesses powers which are not inherent in its separate molecules.*

---

* 'We must, in fact, ascribe an independent life to cells—i.e., the combination of molecules which take place in a single cell are sufficient to set free the force, in consequence of which the cell has the power of attracting new molecules. The cause of nutrition and growth lies not in the organism as a whole, but in the separate elementary parts—the cells. That in point of fact every cell, when separated from the organism, is not capable of further growth, as little militates against this theory as its incapability of existing separate from the swarm would be an argument against the independent life of a bee. The inquiry into the fundamental forces of organisms therefore, is reduced into one concerning the fundamental forces of the single cells.' (p. 229.)

And yet, strongly as Schwann maintains, not only here but in many other places, the view that the vital forces are manifested by the cells as machines, and are not inherent in the matter of which these cells are composed, apart from their form, he gives it up in effect when he comes to treat of these forces in detail. The fundamental cell-forces are, he says, of two kinds, the attractive and the metabolic, the former regulating growth, the latter determining the chemical changes; and he shows very justly that these forces are not located in any special centres in the cell, but are exhibited by all its solid constituents (pp. 233, 236), and that they may be exhibited by different portions of these solid constituents, and to a different extent by these different portions (p. 233), proving hereby, very clearly, as it seems to us, that the forces in question are not centralized in the cells, but are resident in their component molecules. All Schwann's able comparison of cell development with crystallization, tends in fact to the same conclusion. 'When matter crystallizes from a solution, the presence of a foreign body may determine the place and form of the deposit; but the crystals themselves are the result, not of the attractive forces of the foreign body, but of the forces resident in their component molecules. So in cell-development, if it is to be rigorously compared to crystallization, even if the nuclei represent the foreign bodies, which determine the place of the chemical and morphological alterations in the surrounding substance, it by no means follows that they are their cause.

Kolliker (§§ 11, 13), resting especially upon the phenomena of yelk-division and of endogenous cell-development, advocates the existence of a peculiar molecular attraction proceeding from the nucleolus first, and subsequently from the nucleus. Now as regards endogenous cell-development, we must confess that we can find no more ground for its occurrence among animals than among plants. Nageli's cell development around portions of contents, upon which Kolliker lays so much stress, is nothing more than a case of division of the endoplast (primordial utricle) and subsequent development of periplastic substance round the portions In cartilage, which is so often quoted as offering marked endogenous cell-development, we must agree with Leidy and Remak, that nothing but division of the endoplasts (nuclei, primordial utricles) and ingrowth of the periplast (intercellular substance, cell-wall) occurs In these endoplasts again, the very existence of a nucleus is in the highest degree variable and inconstant, and division occurs as well without it as with it

The process of yelk-division—that remarkable manifestation of a tendency to break up, in the yelk of most animals, into successively smaller spheroids, in each of which a nucleus of some kind appears—seems, at first, to offer very strong evidence in favour of the exertion of some attraction by these nuclei upon the vitelline mass But we think that a closer examination completely deprives this evidence of all weight In the first place, the appearance of the nuclei is in many cases subsequent to segmentation It is thus in Strongylus auricularis (Reichert), in Phallusia (Krohn), in the hen's egg (Remak) In the second place, it seems difficult to conceive any mode of operation of a central attractive force which shall give rise to the phenomena of segmentation, for the resulting spheroids always pass into one another by extensive plane surfaces, whereas the even action of two attractive centres, in a mass free to move, would give rise to two spheroids in contact only by a point Again, Remak has observed, that in the frog's egg the time occupied by the formation of the groove, indicating the first line of cleavage upon the upper half of the yelk, is very much shorter than that required to give rise to the corresponding line upon the lower half—a fact which is quite unintelligible upon the theory of a central attraction

Thirdly, in Cucullanus, Ascaris dentata, &c, Kolliker has shown, that though nuclei are developed, no yelk-division occurs, and in the later stages of division of the frog's egg, yelk masses are found undivided, and containing many nuclei

Finally, in Ascaris mystax, according to Dr Nelson,* the embryonic vesicles absolutely revolve in circles during the progress of yelk-division—a phenomenon which seems incompatible with the existence of any mutual attractive reaction between themselves and the vitelline mass

We see, in short, that the effects of the force supposed to be exerted by the nuclei may take place without them, and, on the other hand, that the nuclei may be present without exerting the peculiar forces which they are supposed to possess, and finally, that even if such forces exist, they must be something very different from all the attractive forces of which we have any conception, and therefore that the hypothesis of nuclear force is no explanation, but merely a fresh name for the difficulty

We are as little able to discover any evidence of the existence of metabolic forces in the nuclei The metabolic changes of the tissues—such as we see, for instance, in the conversion of cartilage into bone, of cartilage into connective tissue—do not take place, either primarily or with greater intensity, in the neighbourhood of the nuclei, a fact of which striking evidence is afforded by ossifying cartilage, in which the first deposit of calcareous matter occurs, not in areæ surrounding each nucleus, as we should expect if they exerted a metabolic influence, but in straight lines, which stretch from the ossified surface into the substance of the matrix of the cartilage, and the amount of calcareous matter in which gradually diminishes as we recede from the ossified part, without the least reference to the nuclei It is the same with the metamorphosis of the periplast of cartilage when it passes into tendon.

---

* Philosophical Transactions, 1852

From all this we consider it to be satisfactorily shown, that there is no evidence that the "cells" of living bodies are, in any respect, centres of those properties which are called vital forces What, then, are these cells? it may be asked,—What is the meaning of the unquestionable fact, that the first indication of vitality, in the higher organisms at any rate, is the assumption of the cellular structure?

In answering these questions, we would first draw attention to the definition of the nature of development in general, first clearly enunciated by Von Baer "The history of development," he says, "is the history of a gradually increasing differentiation of that which was at first homogeneous" The yelk is homogeneous, the blastoderma is a portion of it which becomes different from the rest, as the result of the operation of the laws of growth, the blastoderma, again, comparatively homogeneous, becomes differentiated into two or more layers, the layers, originally identical throughout, set up different actions in their various parts, and are differentiated into dorsal and visceral plates, chorda dorsalis and bodies of vertebræ, &c &c No one, however, imagines that there is any causal connexion between these successive morphological states No one has dreamt of explaining the development of the dorsal and visceral plates by blastodermic force, nor that of the vertebræ by chorda-dorsalic force On the other hand, all these states are considered, and justly, to result from the operation of some common determining power, apart from them all—to be, in fact, the modes of manifestation of that power

Now, why should we not extend this view to histology, which, as we have explained, is only ultimate morphology? As the whole animal is the result of the differentiation of a structureless yelk, so is every tissue the result of the differentiation of a structureless blastema—the first step in that differentiation being the separation of the blastema into endoplast and periplast, or the formation of what is called a "nucleated cell"* Then, just as in the development of the embryo, when the blastodermic membrane is once formed, new organs are not developed in other parts of the yelk, but proceed wholly from the differentiation of the blastoderm,—so histologically, the "nucleated cell," the periplast with its endoplast, once formed, further development takes place by their growth and differentiation into new endoplasts and periplasts The further change into a special tissue, of course, succeeds and results from this primary differentiation, as we have seen the bodies of the vertebræ succeed the chorda dorsalis, but is there any more reason for supposing a causal connexion between the one pair of phenomena, than between the other? The cellular structure precedes the special structure, but is the latter, therefore, the result of a "cell-force," of whose existence there is on other grounds no evidence whatever? We must answer in the negative For us the primarily cellular structure of plants and animals is simply a fact in the history of their histological development—a histologically necessary stage, if one may so call it, which has no more causal connexion with that which follows it, than the equally puzzling morphological necessity for the existence of a chorda dorsalis or of Wolffian bodies has, with the development of the true vertebræ or of the true kidneys

---

* Compare Reichert, p. 35

If this be true, we might expect, as we find, that the differentiation of the germinal disc, for instance, into a primitive groove and lateral portions —the first stage of development in the embryo of all vertebrate animals— does not occur in mollusks, as we find, again, that the differentiation of the embryo into plumula and cotyledons which occurs in a great number of plants is absent in others, so, if, like these, the histological differentiation into cells have no necessary causal connexion with the action of the vital forces but be merely a genetic state, we may expect to meet with cases in which it does not occur Such, in fact, are the so called unicellular plants and animals—organisms which often exhibit no small complexity of external form, but present no internal histological differentiation In the genus Caulerpa we have an Alga, presenting apparent leaves, stems, and roots, and yet which, according to Nägeli, consists of a single cell— that is, is not composed of cells at all The Vorticellæ furnish us with examples of animals provided with a distinct œsophagus, a muscular pedicle, &c, and yet in which no further histological differentiation can be made out As Wolff* says—

> "The latter (Roesel's Proteus) has no structure, no determinate figure, and even the indeterminate figure that it has at any given time does not remain the same, but alters continually We can, in fact regard all these plants and animals as little else than living or vegetating matters—hardly as organized bodies
>
> "§ 74 However, all these plants and animals nourish themselves, vegetate, and propagate their species just as well and as easily as the most artificial pieces of mechanism to be met with in the vegetable or animal kingdom"

It is true, indeed, that the difficulty with regard to these organisms has been evaded by calling them "unicellular"—by supposing them to be merely enlarged and modified simple cells, but does not the phrase an "unicellular organism involve a contradiction for the cell-theory? In the terms of the cell-theory, is not the cell supposed to be an anatomical and physiological unity, capable of performing one function only—the life of the organism being the life of the separate cells of which it is composed? and is not a cell with different organs and functions something totally different from what we mean by a cell among the higher animals? We must say that the admission of the existence of unicellular organisms appears to us to be virtually giving up the cell-theory for these organisms If it be once admitted that a particle of vitalizable matter may assume a definite and complex form, may take on different functions in its different parts, and may exhibit all the phenomena of life, without assuming the cellular structure, we think that it necessarily follows that the cells are not the centres of the manifestation of the vital forces, or that, if they be so, the nature of these forces is different in the lower organisms from what it is in the higher—a proposition which probably few would feel disposed to maintain

So much for the critical, and therefore more or less ungrateful, portion of our task We have seen how the great idea, fully possessed by Fallopius, that life is not the effect of organization, nor necessarily dependent upon it, but, on the other hand, that organization is only one of the phenomena presented by living matter—carried to absurdity by Stahl and Van Helmont—has, on the other hand, been too much neglected by the later writers who have attempted to reduce life to the mere attractions and repulsions of organic centres, or to consider physiology simply as a complex branch of mere physics. We have seen how this latter notion has been fostered by the misconceptions of a great botanist, only too faithfully followed in the animal world by the illustrious author of the cell theory, and we have endeavoured to show how the solitary genius of Wolff had kept in the old track, and that the choice of modern histologists lies between him and Schleiden and Schwann. It will be sufficiently obvious that our own election has long been made in this matter, and we beg to submit the following sketch of a general theory of the structure of plants and animals—conceived in the spirit, and not unfrequently borrowing the phraseology, of Wolff and Von Baer.

---

* 'Von der wesentlichen Kraft' p 40

Vitality, the faculty, that is, of exhibiting definite cycles of change in form and composition, is a property inherent in certain kinds of matter.

There is a condition of all kinds of living matter in which it is an amorphous germ—that is, in which its external form depends merely on ordinary physical laws, and in which it possesses no internal structure.

Now, according to the nature of certain previous conditions—the character of the changes undergone—of the different states necessarily exhibited,—or, in other words, the successive differentiations of the amorphous mass will be different.

Conceived as a whole, from their commencement to their termination, they constitute the individuality of the living being, and the passage of the living being through these states, is called its development. Development, therefore, and life are, strictly speaking, one thing, though we are accustomed to limit the former to the progressive half of life merely, and to speak of the retrogressive half as decay, considering an imaginary resting point between the two as the adult or perfect state.*

The individuality of a living thing, then, or a single life, is a continuous development, and development is the continual differentiation, the constant cyclical change of that which was, at first, morphologically and chemically indifferent and homogeneous.

The morphological differentiation may be of two kinds. In the lowest animals and plants—the so called unicellular organisms—it may be said to be *external*, the changes of form being essentially confined to the outward shape of the germ, and being unaccompanied by the development of any internal structure.

But in all other animals and plants, an internal morphological differentiation precedes or accompanies the external, and the homogeneous germ becomes separated into a certain central portion, which we have called the *endoplast*, and a peripheral portion, the *periplast*. Inasmuch as the separate existence of the former necessarily implies a cavity, in which it lies, the germ in this state constitutes a vesicle with a central particle, or a "nucleated cell."

---

* Dr Lyons in his interesting Researches on the primary stages of Histogenesis and Histolysis has invented a most convenient and appropriate term for this latter half of development so far as the tissues are concerned—viz., Histolysis.

There is no evidence whatever that the molecular forces of the living matter (the "vis essentialis" of Wolff, or the vital forces of the moderns) are by this act of differentiation localized in the endoplast, to the exclusion of the periplast, or *vice versâ* Neither is there any evidence that any attraction or other influence is exercised by the one over the other, the changes which each subsequently undergoes, though they are in harmony, having no causal connexion with one another, but each proceeding, as it would seem, in accordance with the general determining laws of the organism On the other hand, the "vis essentialis" appears to have essentially different and independent ends in view—if we may for the nonce speak metaphorically—in thus separating the endoplast from the periplast

The endoplast grows and divides, but, except in a few more or less doubtful cases, it would seem to undergo no other morphological change It frequently disappears altogether, but as a rule, it undergoes neither chemical nor morphological metamorphosis So far from being the centre of activity of the vital actions, it would appear much rather to be the less important histological element

The periplast, on the other hand, which has hitherto passed under the names of cell-wall, contents, and intercellular substance, is the subject of all the most important metamorphic processes, whether morphological or chemical, in the animal and in the plant By its differentiation, every variety of tissue is produced, and this differentiation is the result not of any metabolic action of the endoplast, which has frequently disappeared before the metamorphosis begins, but of intimate molecular changes in its substance, which take place under the guidance of the "vis essentialis," or, to use a strictly positive phrase, occur in a definite order, we know not why

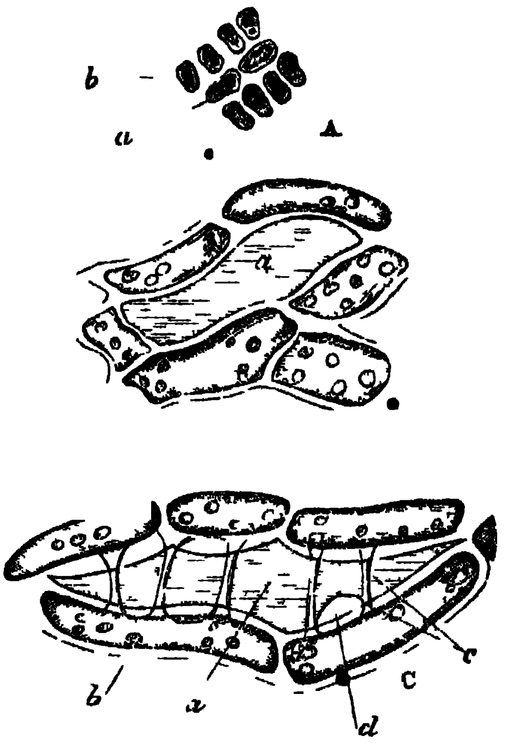
The metamorphoses of the periplastic substance are twofold—chemical and structural The former may be of the nature either of *conversion* change of cellulose into xylogen, intercellular substance, &c, of the indifferent tissue of embryos into collagen, chondrin, &c, or of *deposit* as of silica in plants, of calcareous salts in animals

The structural metamorphoses, again, are of two kinds—*vacuolation*, or the formation of cavities, as in the intercellular passages of plants, the first vascular canals of animals, and *fibrillation*, or the development of a tendency to break up in certain definite lines rather than in others, a peculiar modification of the cohesive forces of the tissue, such as we have in connective tissue, in muscle, and in the "secondary deposits" of the vegetable cell

Now to illustrate and explain these views, let us return to the vegetable and animal tissues, as we left them in describing the base of the Sphagnum leaf and fœtal cartilage, and trace out the modification of these, which are identical with all young tissues, into some of the typical adult forms

The point of the Sphagnum leaf is older than the base, and it is easy to trace every stage from the youngest to the complete forms in this direction At the base of the leaf, we find, as has been said, nothing but minute endoplasts, each resembling the other, embedded in a homogeneous periplastic substance (A), as we trace these upwards, we find that some of the endoplasts increase in size more rapidly than the others (B), and eventually

Fig 2



Portions of the leaf of sphagnum A, from the base B more towards the point C fully formed a endoplasts which disappear b those which remain c spiral thickenings of periplast in the cavities of the former; d apertures formed by resorption

totally disappear, leaving only the endoplastic cavity, or "cell," which contained them In the surrounding cells, the endoplasts are very obvious as granular primordial utricles (C) After the disappearance of the endoplast, changes commence in the periplastic substance or wall of the cell (*a*), more or less circular or spiral thickenings (*c*) taking place in it, so as to form the well-known fibre-cell of the sphagnum leaf, and at the same time, a process of resorption occurs in particular parts of the wall, so that round apertures are formed (*d*) Nothing can be more instructive than this case, the leaf being composed of a single layer of delicate and transparent cells, so that there are no interfering difficulties of observation, and we see demonstrated, in the most striking manner, that the endoplast or primordial utricle has nothing to do with the metamorphoses which occur in the periplastic substance The disappearance of the primordial utricle in cells which are undergoing thickening, was, in truth, long ago pointed out by Von Mohl, but neither he nor any of his successors seem to have noticed how completely this fact does away with that activity of the primordial utricle, and passivity of the cell-wall, which they all assume We have here, in fact, the cell-wall commencing and carrying through its morphological changes after the primordial utricle has completely disappeared, and we see that the so-called secondary deposit in this case, is a morphological differentiation of the periplast, which at the same time exhibits its peculiar powers by setting up a resorption of its substance at another point Here, however, we have no marked chemical differentiation, for an instance of which we may turn to the collenchyma of the beet root (fig 1, A) There is no question that, at one period of its development, the whole periplastic substance here, as in the Sphagnum, was homogeneous, and of the same chemical constitution In the fully formed beet root, however, we have no less than three compounds disposed around each cell cavity The periplastic substance has, in fact, undergone both a chemical and a morphological differentiation—the innermost layers (*c*) consisting of ordinary cellulose, the next of a substance which swells up in water (*b*), and the outermost of a different, but not exactly defined, substance (*a*) We may call one of these portions "cell-membrane," and another intercellular substance, but they are, assuredly, all nothing but differentiated portions of one and the same periplast

Woody tissue presents precisely the same phenomena, the inner layers of the periplastic substance having, very generally, a different composition from the outer

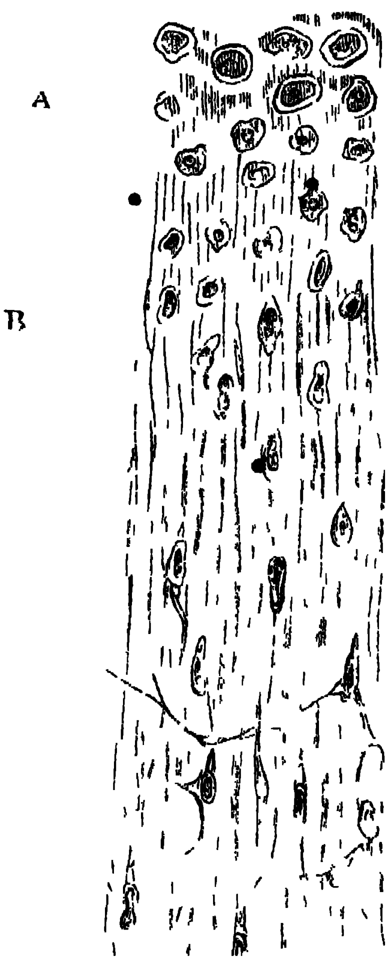
Morphologically, we have already noticed the lamination of the periplastic substance, and we may mention its fibrillation, a process which takes place almost invariably in the inner layers of the periplast, and to which the well-known spirality of the so called secondary deposits must be referred, but a more important process for our present purpose is what we have called Vacuolation, the development of cavities in the periplastic substance independent of the endoplast, and which, to distinguish them from the cells, may conveniently be termed *Vacuolæ* In the youngest vegetable tissues there are no such cavities, the periplastic substance forming a continuous solid whole, and it is by this vacuolation, which occurs as the part grows older, that all the intercellular passages are formed, and that many cells obtain that spurious anatomical independence, to which we have adverted above The exaggerated development of the vacuolæ in the pith of the rush converts the periplastic substance, with its proper endoplastic cavities, into regular stellate cells (Fig 1, B)

Sufficient has been said to illustrate the differentiation of the primitive vegetable structure into its most complex forms If we turn to the animal tissues, we shall find the same simple principles amply sufficient to account for all their varieties

In the plant, as we have seen, there are but two histological elements—the periplastic substance, and the endoplasts, cell wall and intercellular substance, being merely names for differentiated portions of the former, cell-contents, on the other hand, representing a part of the latter In the animal, on the other hand, if we are to put faith in the present nomenclature, we find cell wall, intercellular substance, and cell contents, forming primitive elements of the tissues, and entering into their composition as such there have been no small disputes whether the collagenous portion of connective tissue is intercellular substance or cell-wall, the elastic element being pretty generally admitted to be developed from distinct cells Again, it appears to be usual to consider the fibrillæ of striped muscle as modified cell contents, while the sarcolemma represents the cell-walls The hyaline substance of cartilage is asserted by some to be cell-wall, by some to be intercellular substance, while the walls of the epithelium cavities are admitted, on all hands, to be cell-walls We confess ourselves quite unable to find any guiding principle for this nomenclature, unless it be that the toughest structure surrounding a "nucleus" is to be taken as cell-wall, anything soft inside it being contents, and anything external to it intercellular substance, which is hardly a caricature of the vagueness which pervades histological works upon this subject This results, we think, from the attempt to determine the homology of the parts of the tissues having been made from the examination of their embryonic conditions, where it is often very obscure, and hardly to be made out It is another matter if we adopt the "principle of continuity" of Reichert—a method of investigation which has been much neglected This principle is simply, that whatever histological elements pass into one another by insensible gradations are homologous and of the same nature, and it is so clear and easy of application, that we can but wonder at its hitherto limited use We will now proceed to analyze the nature of the constituents of some of the most characteristic tissues in this way, starting from that of embryonic cartilage, as we have described it above

Connective tissue occurs in two forms,—which, however, pass into one another by infinite gradations—the solid and the areolated of the former we may take a tendon as an example, of the latter, the loose areolar tissue, which is found forming the inner layer of the skin and mucous membranes Fig 3 represents the junction between the tendo Achillis and the cartilage of the os calcis, in a young kitten At A, we have pure cartilage, the endoplasts lying within cavities whose walls present more or less defined contours At B, the cavities and their contained endoplasts are somewhat elongated, and a faint striation is obvious in the upper portion of the periplastic substance, which becomes stronger and stronger, as we proceed lower down, until it ends in an apparent fibrillation A chemical change has at the same time taken place, so that in this portion the striated part of the periplast is swollen up more or less by acetic acid, the walls of the cavities remaining unaffected, and thence becoming more distinct, while in the portion A, the whole periplast was nearly equally insensible to this re-agent The portion C, nearest the tendon, and passing into it, is completely tendinous in its structure The periplast exhibits strong fibrillation, and is very sensitive to acetic acid, while not only the walls of the cavities, but the intermediate periplast, in certain directions, which radiate irregularly from them, have changed into a substance which resists acetic acid even more than before, and is in fact elastic tissue Compare this process with that which we have seen to be undergone by the collenchyma of the beetroot, and we have the fibrillation of the outer portion of the periplast around each endoplast, and its conversion into collagen, answering to the lamination of the "intercellular substance," and its conversion into a vegetable gelatinous matter, while the elastic, corresponds with the cellulose inner wall

Fig 3

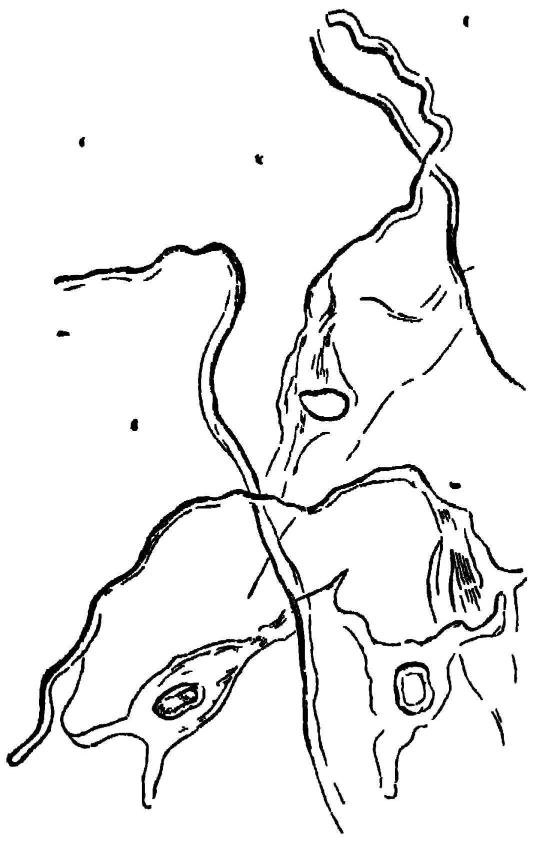


Junction of tendo Achillis and cartilage of the calcaneum in a kitten A Pure cartilage B intermediate portion C tendon It must be understood that the transition is in reality much more gradual the different stages having here been approximated for the sake of economizing space

The testimony of numerous observers agrees that cartilage is converted into connective tissue in the way described Professor Kolliker, who unwillingly admits the fact, suggests, nevertheless, that such connective tissue as this, is not true connective tissue, inasmuch as it presents differences in its mode of development, the collagenous element in the latter being always developed from cells *

Now, we might be inclined to ask, if the substance of the tendo-Achillis is *not* connective tissue, but only "*täuschend ähnlich*," what is? But it is better to attack Prof Kolliker's stronghold, the areolated gelatinous connective tissue, which is, as he justly observes, the early form of fœtal connective tissue generally (l c, p 58) If the outer layer of the corium of the skin, or the submucous gelatinous tissue in the enamel organ, be teased out with needles, we shall obtain various stellate or ramified bodies, containing endoplasts (fig 4), which Kolliker calls cells, and which, as he states, do assuredly pass into and become, bundles of fibrillated connective tissue But is this really a different mode of development from that already described? We think not Indeed, if that portion of this young gelatinous connective tissue, which lies immediately adjacent to the epidermis or epithelium be examined, it will be found to present a structure in all respects similar to fœtal cartilage, that is, there is a homogenous matrix in which the endoplasts are dispersed (fig 5 B) If this be traced inwards, it will be found, that the endoplasts become more widely separated from one another, and that the matrix in places between them is softened and altered, while in their immediate neighbourhood, and in the direction of irregular lines stretching from them, it is unaltered This is, in fact, the first stage of that process which we have called vacuolation In this condition the intermediate softened spots still retain sufficient consistence not to flow out of a section, but yielding, as it does, in these localities, much more readily than in others, it is easy enough to tear out the firmer portion in the shape of "cells," which are fusiform, irregular, or stellate, and the whole tissue has therefore been described (Reichert, Virchow, Schwann) as consisting of cells, connected by an 'intercellular substance Both "cell-walls" and "intercellular substance," however, are portions of the same periplast, and together correspond with the matrix of the cartilage When, therefore, in the course of further development, the "intercellular substance" becomes quite fluid and so disappears, the outer portion of these cells being converted into fibrillated collagenous tissue,

Fig 4

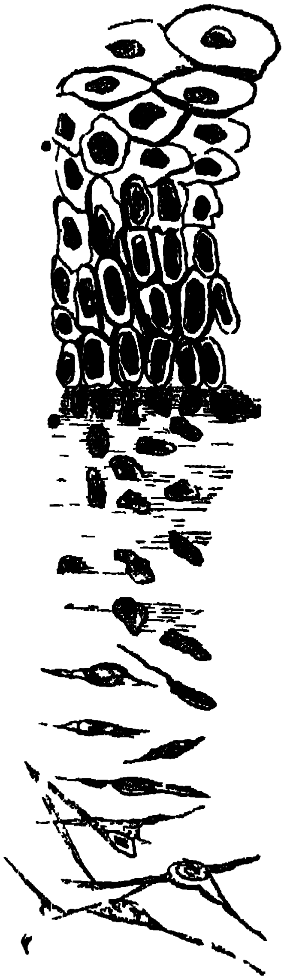


Stellate cells of young connective tissue from the actinenchyma of the enamel organ of the calf and the inner into elastic substance, we have, notwithstanding the apparently great difference, in reality exactly the same mode of metamorphosis of the same elements, as in the preceding instance Connective tissue, therefore, we may say, consists in its earliest state of a homogeneous periplast inclosing endoplasts The endoplasts may elongate to some extent, but eventually become lost, and cease, more or less completely, to be distinguishable elements of the tissue The periplast may undergo three distinct varieties of chemical differentiation, e g, into the gelatinous "intercellular substance," the collagenous "cell-wall," and the elastic "cell wall," and two varieties of morphological differentiation, vacuolation, and fibrillation—and the mode in which these changes take place gives rise to the notion that the perfect tissue is composed of elements chemically and mechanically distinct

---

* Handbuch, pp 58, 59, 218

Fig 6



Submucous tissue and epithelium of the tongue of the kitten A Epithelium, B young connective tissue

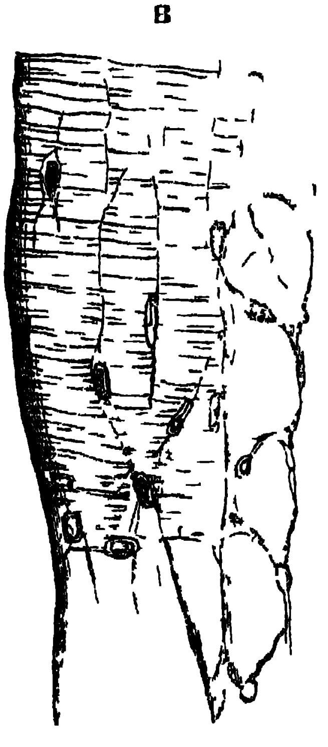
The proper understanding of the nature and mode of development of the component parts of connective tissue is, we believe, of the first importance in comprehending the other tissues If we clearly bear in mind, in the first place, that the periplast is capable of undergoing modifications quite independently of the endoplasts, and secondly, that in consequence of their modification, elements may become optically, mechanically, or chemically separable from a perfect tissue, which were not discoverable in its young form, and never had any separate existence, many of the great difficulties and perplexities of the cell-theory will disappear Thus, for instance, with regard to the structure of bone, there can be no doubt that the "nuclei" of the corpuscles are endoplasts, and that the calcified matrix is the periplast This calcified matrix has, however, in adult bone, very often a very regular structure, being composed of definite particles To account for these, Messrs Tomes and De Morgan, in their valuable essay on ossification, which has just appeared,* suppose that certain "osteal cells" exist and become ossified We have no intention here of entering upon the question of the existence of these "osteal cells" as a matter of fact, but we may remark, that they are by no means necessary, as the appearance might arise from a differentiation of the periplast into definite particles, corresponding with that which gives to connective tissue its definite and fibrillated aspect So with regard to the vexed question whether the lacunæ have separate parietes or not, how readily comprehensible the opposite results at which different observers have arrived become, if we consider that their demonstrability or otherwise results simply from the nature and amount of the chemical difference which has been established in the periplast in the immediate neighbourhood of the endoplast, with regard to that in the rest of the periplast In fig 3 substitute calcific for collagenous metamorphosis, and we should have a piece of bone exhibiting every variety of lacunæ, from those without distinct walls, to those which constitute regular stellate "bone corpuscles" Finally, in bone, the formation of the "Haversian spaces" of Tomes and De Morgan is a process of vacuolation, strictly comparable to that which we have described as giving rise to the areolated connective tissue Cancellated bone is, in fact, areolated osseous tissue Once having comprehended the fact that the periplast is the metamorphic element of the tissues, and that the endoplast has no influence nor importance in histological metamorphosis, there ceases to be any difficulty in understanding and admitting the development of the tubules of the dentine and the prisms of the enamel, without the intervention of endoplasts These are but extreme and obvious cases in which nature has separated for us two histological elements and two processes, which are elsewhere confounded together

---

* Phil. Trans, 1853

One of the most complicated of tissues is striped muscle, yet the true homology of its elements seems to us to become intelligible enough upon these principles Dr Hyde Salter has pointed out,* that in the tongue the muscles pass directly into the bundles of the submucous connective tissue which serve as their tendons We have figured such a transition in fig 6 The tendon A may be seen passing insensibly into the muscle B, the granular sarcous elements of the latter appearing as it were to be deposited in the substance of the tendon (just as the calcareous particles are deposited in bone), at first leaving the tissue about the walls of the cavities of the endoplasts, and that in some other directions, unaltered These portions, which would have represented the elastic element in ordinary connective tissue, disappear in the centre of the muscular bundle, and the endoplasts are immediately surrounded by muscle, just as, in many specimens of bone, the lacunæ have no distinguishable walls On the other hand, at the surface of the bundle the representative of the elastic element remains and often becomes much developed as the sarcolemma There is no question here of muscle resulting from the contents of fused cells, &c It is obviously and readily seen to be nothing but a metamorphosis of the periplastic substance, in all respects comparable to that which occurs in ossification, or in the development of tendon In this case we

Fig 6

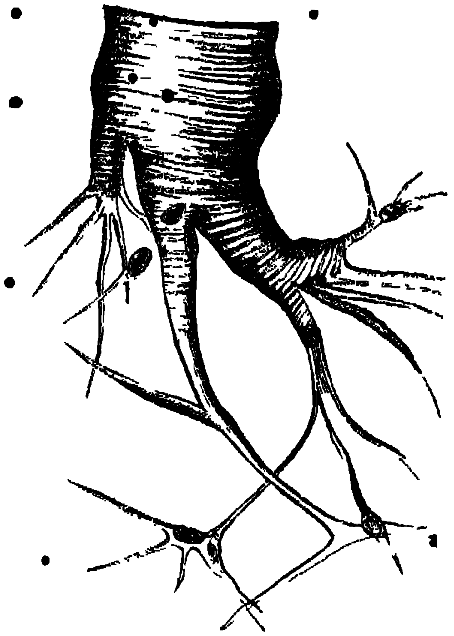


Continuity of muscle with connective tissue from the tongue of the lamb A Connective tissue, B muscle might expect, that as there is an areolar form of connective tissue, so we should find some similar arrangement of muscle, and such may indeed be seen very beautifully in the terminations of the branched muscles, as they are called In fig 7 the termination of such a muscle, from the lip of the rat, is shown, and the stellate "cells" of areolated connective tissue are seen passing into the divided extremities of the muscular bundle, becoming gradually striated as they do so

---

* Art 'Tongue,' Todd's Cyclopædia

Fig 7



Branched muscle ending in stellate connective cells from the upper lip of the rat

We have already exceeded our due limits, and we must therefore reserve for another place the application of these views to other tissues There is, however, one application of the mode of termination of the branched muscles to which we have just referred, which is of too great physiological importance to be passed over in silence In the muscle it is obvious enough, that whatever *homology* there may be between the stellate "cells' and the muscular bundles with which they are continuous, there is no *functional analogy*, the stellate bodies having no contractile faculty But a nervous tubule is developed in essentially the same manner as a muscular fasciculus, the only difference being, that fatty matters take the place of syntonin Now, it commonly happens that the nerve tubules terminate in stellate bodies of a precisely similar nature, and these, in this case, are supposed to possess important nervous functions, and go by the name of "ganglionic cells" From what has been said, however, it is clear that these may be genetically and not functionally, connected with the nervous tubules, and that, so far from being the essential element of the nervous centres and expansions, it is possible that the "ganglionic cells" have as little nervous function, as the stellate cells in the lip of the rat have contractile function

We cannot conclude better than by concisely repeating the points to which we have attempted to draw attention in the course of the present article

We have endeavoured to show that life, so far as it is manifested by structure, is for us nothing but a succession of certain morphological and chemical phenomena in a definite cycle, of whose cause or causes we know nothing, and that in virtue of their invariable passage through these successive states, living beings have a development, a knowledge of which is necessary to any complete understanding of them It has been seen that

Von Baer enunciated the law of this development, so far as the organs are concerned, that it is a continually increasing differentiation of that which was at first homogeneous, and that Caspar Friedrich Wolff demonstrated the nature of histological development to be essentially the same, though he erred in some points of detail. We have found Schwann demonstrating for the animal, what was already known for the plant—that the first histological differentiation, in the embryo, is into endoplast and periplast, or, in his own phrase, into a "nucleated cell," and we have endeavoured to show in what way he was misled into a fundamentally erroneous conception of the homologies of these two primitive constituents in plants and animals—that what he calls the "nucleus" in the animal is not the homologue of the "nucleus" in the plant, but of the primordial utricle.

We have brought forward evidence to the effect, that this primary differentiation is not a necessary preliminary to further organization—that the cells are not machines by which alone further development can take place, nor, even with Dr Carpenter's restriction (p 737), are to be considered as "instrumental" to that development. We have tried to show that they are not instruments, but indications—that they are no more the producers of the vital phenomena, than the shells scattered in orderly lines along the sea-beach are the instruments by which the gravitative force of the moon acts upon the ocean. Like these, the cells mark only where the vital tides have been, and how they have acted.

Again, we have failed to discover any satisfactory evidence that the endoplast, once formed, exercises any attractive, metamorphic, or metabolic force upon the periplast, and we have therefore maintained the broad doctrine established by Wolff, that the vital phenomena are not necessarily preceded by organization, nor are in any way the result or effect of formed parts, but that the faculty of manifesting them resides in the matter of which living bodies are composed, as such—or, to use the language of the day, that the "vital forces" are molecular forces.

It will doubtless be said by many, But what guides these molecular forces? Some Cause, some Force, must rule the atoms and determine their arrangement into cells and organs, there must be something, call it what you will—Archæus, "Bildungs trieb," "Vis Essentialis," Vital Force, Cell-force—by whose energy the vital phenomena in each case are what they are.

We have but one answer to such inquiries. Physiology and Ontology are two sciences which cannot be too carefully kept apart, there may be such entities as causes, powers, and forces, but they are the subjects of the latter, and not of the former science, in which their assumption has hitherto been a mere gaudy cloak for ignorance. For us, physiology is but a branch of the humble philosophy of facts, and when it has ascertained the phenomena presented by living beings and their order, its powers are exhausted. If cause, power, and force, mean anything but convenient names for the mode of association of facts, physiology is powerless to reach them. [illegible] is satisfactory to reflect, however, that in this comparatively limited sphere the inquiring mind may yet find much occupation.

T H Huxley

REVIEW II

1 *Observations on Intestinal Obstructions depending on Internal Causes, and on the Means to be employed for their Relief* By BENJAMIN PHILLIPS, F R S ('Med -Chir Trans' vol 31)

2 *Case of Structure of the Colon, successfully treated by Operation, with an Analysis of Forty-four Cases of Artificial Anus* By CÆSAR HAWKINS, Senior Surgeon to St George s Hospital, and President of the Royal College of Surgeons of England ('Med.-Chir Trans' vol 35)

3 *Remarkable Case of Foreign Bodies in the Stomach and Duodenum, Complete Obstruction of the Bowel, and Mechanical Displacement of Organs* By JOHN MARSHALL, Esq ('Med -Chir Trans' vol 35)

4 *Two large masses of Hair and String matted together, taken from the Stomach and Duodenum, and which produced Obstruction and Death.* By GEORGE POLLOCK ('Trans Patho Soc of London.' 1852-53 p 327)

5 *A Case of Constipation of Nine Days' duration from Scirrhous Rectum, relieved by Opening the Sigmoid Flexure of the Colon in the Left Groin* By JOHN ADAMS, Surgeon to the London Hospital ('Med -Chir Trans' vol 35)

6 *An Account of Two Cases of Intestinal Obstructions, in which the Operation for the Formation of an Artificial Anus was performed—one in the Ascending, the other in the Descending, Colon* By WILLIAM J CLEMENT, of Shrewsbury, F R S ('Med -Chir Trans' vol 35)

7 *A Case of Intestinal Obstruction from Disease of the Rectum, treated successfully by Opening the Descending Colon in the Left Loin* By ALFRED BAKER, Surgeon to the General Hospital, Birmingham. ('Med -Chir Trans' vol 35)

8 *Case of Intestinal Obstruction* By J LUKE, Vice-President of the Royal College of Surgeons of England, and Senior Surgeon to the London Hospital

9 *Intestinal Obstruction of Twelve Days' duration from Structure of the Ascending Colon, with Operation for Inguinal Hernia, supposed to be Strangulated* By ALEXANDER SHAW, Esq , Surgeon to the Middlesex Hospital ('Trans Patho Soc of London' 1852-53)

10 *Internal Strangulation of a portion of the Small Intestine by a strong Fibrous Band connected with the Mesentery* By N WARD, Esq , Assistant-Surgeon to the London Hospital ('Trans Patho Soc of London' 1852-53)

MR. PHILLIPS, has observed in the commencement of his paper, which stands first on our list, that—

> "At this advanced period in the history of medicine, it might reasonably be supposed that a disease must be either very insignificant in its consequences, or of a very uncommon occurrence, if it have escaped very ample, and it may be sufficient, consideration, and no doubt the expectation would be usually realized But there

are diseases which are neither uncommon nor insignificant, and yet their history remains to be told, and of the number, are obstructions of the bowels, dependent on internal mechanical causes."

Since the publication of the paper from which the foregoing extract has been borrowed, the medical periodicals and the medical societies have repeatedly furnished us with the particulars of cases which bear upon this subject, accompanied with many most valuable observations, especially illustrating certain points of treatment. Such cases, and such observations, clinically considered, are of the utmost importance, when any additional facts or suggestions, in relation to a subject surrounded with so much that is obscure and difficult, must be received with satisfaction, and with a hope that something more valuable may follow.

Under present circumstances, and with our past experience, should we be right in considering that the "history" of this class of cases has been rendered complete?—Are we wrong if we still consider that "their history remains to be told"? To Mr Phillips and to Mr Cæsar Hawkins the thanks of the profession are eminently due for having especially drawn attention to this subject, by their excellent communications to the Medico-Chirurgical Society. With every wish to render all credit to those others, who have devoted any attention to the subject of obstructions of the intestines, dependent on internal causes, we venture to hope that we shall not have wasted the time of our readers, nor employed our own to disadvantage, should we endeavour to add something to a chapter in their history, by entering into an examination of the subject, and sketching out some general arrangements of the causes of these obstructions, accompanied with the observations lately brought to our notice with respect to the remedies by which these obstructions are to be met.

The consideration of all the various interesting and important practical points, in connexion with the cases and communications which introduce this article to our readers, accompanied with an examination as complete and satisfactory as our inclination would dictate, or as the subject demands, should embrace the consideration of all that is known of the pathology and treatment of obstructions of the intestinal canal—all that has come within our experience and observation—and much that may be found in the medical literature of the past and present period. But on this occasion, we cannot hope, as neither our space nor our time will allow us, to chronicle all that is concerned in the origin, progress, treatment, and termination of the varieties of these affections.

Nor do we consider that any apology is necessary for introducing in a prominent position, and for special notice, such a subject in such a manner, particularly as the attention and eminent experience of many surgeons have been so happily directed to the consideration of some of the difficulties inherent to these cases, and as there has been added by them something that is valuable to this division of practical medicine. Only within a comparatively recent period has any great attention been directed to this class of cases. As regards the treatment of many of them, this remark most especially holds good; and though, formerly, pathological knowledge and practical experience, no doubt in certain instances correctly, diagnosed the causes of obstruction, surgeons had not yet been emboldened to carry out measures which now stand approved of, by the results of experiment, and are not only considered serviceable, but essential in treatment, as tested by the beneficial effects of their application

In the consideration of the causes of obstruction of the intestinal canal dependent on certain internal mechanical or diseased conditions, it is necessary to dismiss, as foreign to our division of the subject, those causes of obstruction dependent on the varieties of umbilical, femoral, or inguinal herniæ

We reserve for our investigation—Firstly, Those causes of obstruction which originate in and implicate the muscular and mucous walls of the intestine Secondly, Those causes which act from without, or affect the serous covering of the bowel Thirdly, Those collections of matter, or lodgments of foreign substances, which occur within and obstruct the passage of the bowel

Each of these divisions possesses its own variety of diseases, and it may be more satisfactory to have them placed before us, previous to the consideration of that most important question—viz, the question involving the amount of relief that can be offered, or the remedy that can be applied, in the individual cases

In the first division, in which the causes of obstruction originate in or implicate the muscular or mucous walls of the intestine, we find—

a Cancer—cancerous stricture
b Contraction of cicatrices following ulcerations
c Contractions of walls of intestine, the result of inflammation, non cancerous deposit, or injury
} Non-cancerous stricture
d Non cancerous growths, in the form of polypi, projecting into and obstructing the cavity of the intestine
e Intus-susception and its results

In the second division, in which the causes act from without, or affect the serous covering of the bowel, we observe—

a Effusions of lymph, in the form of bands or layers, by contraction pressing on the intestine, and producing obstruction
b Loops or openings, formed by adhesions of omentum or intestine, "holes or fissures (congenital or acquired) in mesentery,"* &c, in which portions of bowel become entangled and strangulated
c Effusions of lymph, by which portions of intestine become adherent to each other at acute angles
d Diverticula of the small intestines, either by their own adhesions or the adhesions of cords frequently attached to them, producing pressure, or loops in which bowel becomes strangulated
e Peritonitis—local, general, scrofulous, &c
f Strangulations through natural openings—foramen of Winslow, &c
g Tumours or abscesses pressing upon, or implicating secondarily, the structure of the intestine

In the third division, in which collections of matter or lodgments of foreign bodies occur within and obstruct the bowel, we may place—

a Fæculent matter
b Calculi
c Foreign bodies swallowed by accident, or under other circumstances

---

* Rokitansky, vol ii p 53

There are also some additional causes which cannot be included under any one of these divisions, to which an independent and separate consideration should be given, and which should rather be appended to the foregoing than be placed with them The following appear to us the exceptional causes

1 Displacements or twistings of the bowels
2 Congenital malformations, causing obstructions
3 Ileus

That these divisions are open to certain objections there is no doubt—both anatomically and pathologically considered, they are neither so accurate nor so defined as they otherwise might be, did our space at present allow of more than a general review of these affections The highest aim of medical philosophy is the endeavour to examine into and unravel the causes of disease, before it can satisfactorily contemplate its effects, or hope to apply to those effects measures of a remediable or palliative nature At present, however, we wish rather to consider the ascertained effects than the causes, in conjunction with those measures by which these effects may be removed, or rendered less In greater detail, and for more minute accuracy, we should necessarily examine into the particular characteristics of each form of disease—and on a future occasion we hope to do so to some extent—but, as it is, the above divisions appear to us ample for our purpose, and satisfactory for the elucidation of our subject-matter, in fact, equivalent for the illustration of the most important points occurring in the cases selected for our notice

One important point to which an investigation must be directed, and which in importance is co-equal to an accurate knowledge of the nature and situation of the disease itself, is the *relative frequency* of the various diseases or disorders met with in the human subject giving rise to these obstructions—a point most important in assisting us to decide upon the probability of relief or remedy—and in allowing us to assume a favourable or adverse prognosis In commencing this investigation, let us first consider the causes of obstruction mentioned by Mr Phillips He states, that in 169 cases of obstruction, the causes may be divided in the following manner

		No of cases
1	From invagination of the intestine	63
2	From tumours pressing upon ditto	16
3	From stricture, the result of disease of the parietes	19
4	From infra intestinal tumours, collections of fæces, or concretions	11
5	From constrictions, bands, adhesions, strangulations through abnormal openings, or by twists of intestine	60
		169

Of these 169 cases Mr Phillips states that he found 136 on record We have classed them according to his own arrangement and division, but we must confess that we have some hesitation in offering a collection of cases thus arranged as one from which we should be inclined to draw accurate or satisfactory deductions—either, in the first place, as to the causes of obstructions, or, in the second place, as to the relative frequency of occurrence of such cause, or, lastly, as to the considerations of treatment For, as to causes, we find too many varieties placed together, as to the relative frequency of occurrence of each cause, the numbers do not therefore assist us in deciding how often most of the causes were met with, and as to considerations of treatment, the want of accurate minutiæ in the former would necessarily confine or render somewhat indefinite the principles of the latter.

We trust we shall hereafter be able to point out how very desirable an object it is to obtain something like accuracy of detail in the consideration of each cause of obstruction. It must not be supposed that by these observations, indicating a certain deficiency, we wish to detract from the merit due to Mr Phillips for the valuable paper with which he has furnished us. He has collected materials far and near, and has constructed from them, and from his own experience and opportunities of observation, that which well deserves, not only the consideration, but the commendation of the profession.

Our chief object in this, as in every other inquiry in medicine, must be an approach towards some general principle with regard to the manner in which we should view this class of cases, so that we may apply our remedies with something like precision, and a prospect of success. But a principle without facts, in our department of medicine, would be something analogous to the superstructure built upon a sandy foundation, whereas the facts to establish the principle must be authenticated and accurate, otherwise they would, as a foundation upon which to build our principles, be as useless as the sand itself.

Mr Cæsar Hawkins has drawn up a table of cases, in which altogether 47 instances of obstructed bowel are recorded, and in which record, careful examination and accuracy of detail provide us with much more satisfactory evidence respecting the actual disease that produced, and the relative frequency of the cause of, the obstruction. We find the causes to be as thus divided.

			No of cases
1	From cancerous stricture	18	20
2	From cancer of sigmoid flexure or omentum	1	
3	Adhesion of ileum and rectum to cancerous uterus	1	
4	From stricture supposed to be non cancerous		20
5	From twist of the intestine		1
6	From band producing strangulation		1
7	From adhesion of rectum to uterus after abscess		1
8	From fibrous tumour pressing on rectum		1
9	From ulceration of rectum and contraction		1
10	Unknown and doubtful		2
			47

The published 'Transactions' of the Pathological Society of London have furnished us with the particulars of 32 cases, in which various causes of obstruction existed, and in most of them the obstruction was the cause of death. None of these 32 cases have been included in Mr Hawkins' list, and that we may follow out an accurate investigation of them, we have tabulated them together in a condensed form, in order that reference may be readily made to them by our readers, while they peruse our examination of the causes of obstruction therein found and described.

TABLE OF THIRTY-TWO CASES OF INTESTINAL OBSTRUCTION

No	Sex	Age	Reference to account of case	Disease or Cause of Obstruction	Symptoms	Remarks.
1	F	46	Pathol Trans vol i p 66, Dr Barlow	Stricture of rectum ten inches from anus fish bone lodged in it admitted passage of little finger	Complete constipation nineteen days before death passed little water and with pain constant vomiting peritonitis	General health good until two months before death when suffered from constipation and difficulty in passing motions
2	M.	16	Ibid vol i p 67 Mr Busk.	Stricture of rectum from medullary sarcoma deposited external to muscular fibres pressing on intestine, three to four inches from anus ulceration of mucous membrane	Death from peritonitis of acute character other symptoms not mentioned	
3	F	34	Ibid. vol i p 68 Dr O Ward & Mr Hewett	Fibrous polypus of size and shape of pear connected by long pedicle to inner surface of bowel and invagination of small intestine about three feet from cæcum	Vomiting and constipation seven days previous to death no peritonitis	Invaginated portion easily drawn out no marks of inflammation on surface mucous membrane dull and grey and of gangrenous odour immediately below invagination the polypus was attached and the tumour was not, therefore, apparently connected with the invagination
4	M.	46	Ibid vol i p 68 Mr Hewett	Fibrous polypus of intestine, and invagination of ileum two feet from the cæcum	Obstinate constipation of eight days duration was followed by diarrhœa, which continued till death peritonitis escape of fæculent matter	Above the invagination there was enormous dilatation of intestine and below slight contraction the polypus depended from the invaginated portion of gut into the cavity of the portion below not invaginated The polypus was two and a half inches long and one and a half in diameter, and attached by pedicle to the inner surface of the bowel The peritoneal surfaces of the invaginated portion had firmly united and the portion itself was dark and livid In fact, it had nearly separated with the polypus attached from the gut immediately above the invagination as a large ulcer passing two-thirds round the inner surface of the bowel had almost allowed this to occur

5	M	65	Ibid vol i p 70 Dr Peacock.	Portion of ileum strangulated by passing through opening formed by fibrous band attached to omentum and caput cæci and Poupart s ligament oval polypus the size of a marble, attached by broad base to inner surface about two inches above cæcum and so encroached on canal that the forefinger could barely pass	Constipation six days before death, peritonitis	The adhesions in this case were numerous, above the contraction there was much distension of the bowel *A long band of omentum had furrowed another portion of bowel*
6	F	67	Ibid. vol i p 73 Mr Hewett.	Obturator hernia complicated with reducible femoral hernia	Stercoraceous vomiting symptoms of strangulation six days before death	Old hernial sac, small size laid open but found empty during life narrow opening leading from it into cavity of peritoneum Two-thirds diameter of small intestine eight feet above cæcum strangulated in left obturator foramen
7	M	28	Ibid vol i p 76 Mr Busk	Twisting of sigmoid flexure of colon	Constipation five days before death had suffered from previous attacks of constipation	Sigmoid flexure occupied the greater portion of the abdomen and was gangrenous *rupture of the serous coat* before death
8	M	65	Ibid. vol i p 76 Mr Pollock.	Twisting of the sigmoid flexure of colon	Tendency to diarrhœa at first; constipation not complete great distension great pain and constant vomiting, peritonitis	Sigmoid flexure occupied the greater portion of the abdomen mucous membrane of sigmoid portion dark livid colour the part that was twisted still allowed slight communications with upper and lower portions of bowel
9	M (a sailor)		Ibid. vol i. p 77 Dr R Quain	Intus-susception of ileum six inches in length near cæcum	No account to be obtained beyond his lying down in railway carriage while on a journey and found dead at the end of it	Portion of bowel intus-suscepted in a gangrenous condition
10	F	27	Ibid vol i p 255 Dr Peacock.	Biliary calculus lodged in ileum having escaped through ulcerated opening between gall bladder and duodenum	Constipation six days, vomiting, peritonitis	The adhesions between gall bladder and duodenum gave way while vomiting and contents of bowel escaped into peritoneum
11	M	36	Ibid. vol i p 262, Dr Fuller	Displacement of sigmoid flexure, and enormous distension of bowel obstruction as if from want of muscular action	Constipation seven days before death, subsequent to previous dysentery, vomiting, peritonitis	The sigmoid flexure filled the greater part of the abdomen The mucous membrane of this portion most extensively destroyed by old ulceration

TABLE OF CASES—*continued*

No	Sex	Age	Reference to account of case	Disease or Cause of Obstruction	Symptoms.	Remarks
12	F	68	Pathol Trans., vol i. p 265 Dr Thurnam.	Cancerous stricture of transverse colon involving muscular and mucous coats	Constipation with complete obstruction fæcal matter and gas passed through fistulous opening blood in motions	Fistulous openings formed during life externally through abdominal wall with portion of bowel above stricture patient lived five months afterwards stricture barely allowed little finger to pass after death
13	M	35	Ibid vol. i p. 267 Mr W Adams	Colloid cancer of rectum	Constipation sometimes diarrhœa fæcal vomiting great pain	Transverse colon dragged down and adherent to diseased mass sigmoid flexure remarkably twisted and involved in the deposit transverse colon sigmoid flexure and rectum each opened into cavity formed by mass of colloid deposit.
14	M	28	Ibid	Ileum strangulated through loop formed in omentum	Constipation five days peritonitis	Bowel perforated by ulceration
15	M	4½	Ibid vol ii p 63 Dr Hare.	Intus susception of ileum through ileo-cæcal valve into colon for three inches and a half, firmly held by valve	Constipation four days before death sudden pain blood passed by motions constant desire to go to stool intense abdominal pain vomiting *ascarides*	The appendix was carried through the ileo-cæcal valve with the intus suscepted portion of ileum *The strangulated portion was so firmly held by the valve and so swollen that reduction appeared impossible without tearing* the bowel a second intussusception existed higher up of slight extent
16	Child	6 m.	Ibid vol ii p 56 Mr E Smith	Intus-susception of cæcum ascending and transverse colon into sigmoid flexure	Obstinate constipation vomiting bloody mucous discharge from rectum sixty hours before death	The mucous membrane was purple from strangulation.
17	F	22	Ibid vol ii. p 56 Dr Peacock	Stricture of sigmoid flexure of colon	Habitual constipation and dyspepsia for several months sudden pain and vomiting constipation rather more than three weeks before death tenderness and fulness in course of descending colon some scybala passed with enema, and several dark fœtid motions with shreds like membrane peritonitis	Colon was of deep brown colour and distended from cæcum to sigmoid flexure coats of intestine at strictured portion much thickened and indurated and upper part of stricture surrounded by large deep ulcer From this point to cæcum the mucous membrane was gangrenous

18	F	18	Ibid vol ii p 58 Mr Hewett	A pin covered with calcareous matter lodged in the appendix cæci portion of cæcum and appendix attached to pelvis	Not particularly mentioned death appeared rather sudden bowels enormously distended no peritonitis	Occupying the cæcum was a portion of the pin surrounded by calcareous matter like a large-headed nail the remainder of it being in the appendix The cæcum and appendix were both perforated by ulceration
19	M	30	Ibid vol ii p 60 Dr Peacock	Meso-colic hernia	None death from typhus	On the abdomen being opened none of the small intestines were to be seen they were concealed in a sac formed in the folds of the meso-colon behind the transverse colon which dipped down towards the brim of the pelvis
20	M	27	Ibid vol ii p 60 Dr Peacock	Meso-colic hernia	Pain in abdomen vomiting of fæcal character constipation forty one hours after symptoms commenced death	Descending colon pushed to right side lying close to cæcum The small intestine contained in sac formed by folds of left meso-colon The ileum passed out of sac about two inches above cæcum, and at that point was contracted thickened and gangrenous
21	M	20	Ibid vol ii p 62, Mr Avery	Constriction of ileum by very slight process of membrane attached between the mesentery of ileum and vermiform process	Abdomen greatly distended stercoraceous vomiting constipation fifteen days before death had suffered several times previously from painful attacks of the bowels, *no peritonitis*	Small intestines greatly distended and the large contracted near termination of ileum was a portion of it two feet long much distended and darker than any other portion and firmly constricted at its upper and lower portions especially at the latter where it was crossed by the band
22	M	50	Ibid vol ii p 218 Mr Ward	Stricture of rectum about twelve inches from anus complicated with old reducible hernia	Constipation nine days before death uneasiness about abdomen vomiting on fifth day while vomiting felt his rupture descend suddenly but after three or four hours he managed to return it	As it was considered that the hernia might have been reduced *en masse* Mr Luke made an exploratory incision into the inguinal canal and opened the abdomen at the upper and outer part of the internal ring but as nothing was observed to indicate obstruction the operation was terminated An ordinary sized probe could only be passed through the stricture
23	F	47	Ibid vol iii p 94 Mr Stanley	Obturator hernia complicated with femoral hernia (omental)	Constipation three weeks before death stercoraceous vomiting as femoral hernia existed an operation was performed for its relief but a portion of omentum was only found in it, not strangulated	A portion of ileum surrounded by omentum was found strangulated in the obturator foramen bowel not sufficiently constricted to cause its strangulation and some doubt was expressed by author of case whether this was the sole cause of the constipation

TABLE OF CASES—*continued*

No	Sex.	Age	Reference to account of case	Disease or Cause of Obstruction	Symptoms	Remarks.
24	F	34	Pathol Trans vol iii p 95 Mr Obré	Stricture of ileum a little above cæcum following shortly after strangulated femoral hernia which was relieved at the time by operation	Six months after operation flatulence and pain after food occasional vomiting bowels acted but abdomen increased in size enormous distension of bowels at last constant vomiting	The small intestines were filled to the utmost they were lying vertically in front of the other viscera a few dark points of gangrene on them one had given way The contraction about twelve inches from cæcum at this point the intestine was so thickened that water would only drip through it This portion had apparently adhered to the inguinal ring but from its altered position and distension the adhesion was stretched into a band seventeen and a half inches long
25	M	42	Ibid. vol iii p 101 Mr Gay	Internal strangulation of small intestine between appendix adherent to ileum and band of false membrane Gastrotomy	Pain in abdomen vomiting and constipation five days before operation fæcal vomiting on fifth day abdomen hard and tumid had suffered from several previous attacks of constipation and sickness	Died morning after operation of opening abdomen when the strangulation was relieved sickness ceased but no evacuation after operation recent peritonitis seen after death The strangulated portion had been quite relieved by the operation.
26	M	34	Ibid vol iii p 106 Mr Callaway	Enormous dilatation of sigmoid flexure of colon	Sickness pain and tension of abdomen bowels acted freely after some medicine but on the second day distension increased no amount of action of bowels six days before death long tube relieved bowel of flatus but no mention of fæces passing through it	In this case there is no account of twist, displacement or constriction but obstruction from some cause produced similar effects The bowel was punctured by a fine trochar needle and gave great relief nor did its use indicate any bad effects, the punctures being hardly visible after death The coats of the whole large intestine were thickened *very small quantity of lymph was smeared over intestinal wall*
27	F	46	Ibid vol iii p 111 Dr Hare	Strangulation of five feet of ileum in opening formed by adhesion of omentum to parietes of abdomen adhesion of small intestine to cancerous uterus, producing secondary slight constriction of bowel	Vomiting griping pains constipation though not quite complete very considerable for some five weeks before death	The sub-peritoneal tissue was infiltrated with granules of cancerous matter most abundant in pelvis and probably influencing the adhesion of the omentum the latter was folded up into a flattened cord like body

28	F	18	Ibid vol iii Mr Pollock	Large collections of hair and string matted together and impacted in the stomach and small intestine the duodenum and upper portion of jejunum	Constant vomiting and difficulty in partaking of solid food pain in abdomen tumour *which was moveable*, to be felt in epigastrium	The stomach was much enlarged and the upper portion of small intestine much distended she appeared healthy and had not been observed to eat hair since a child when she was in the habit of putting it into her mouth Peritonitis was general
29	M.	75	Ibid vol iii	Cancerous stricture of colon complicated with omental inguinal hernia for the latter operation performed	Twelve days before seen the hernia descended the vomiting commenced and the constipation became evident during operation passed a small quantity of fæces but subsequently bowels did not again act urine of usual quantity, death four days after operation	The stricture was situated at the commencement of the transverse arch it was an inch and a half in length and so narrow that a goose-quill could not pass through it Cancerous matter was deposited in and around the walls of intestine *The colon beyond the stricture was loaded with large lumps of fæces*
30	F	6	Ibid vol iii p 362 Mr Ward	Strangulation of small intestine by a ring formed by adhesion of false membrane	Constant vomiting and constipation four days before death not much pain	Intestine was dark, livid and lustreless slight recent peritonitis
31	F	37	Ibid. vol iii p 365 Mr Ward	Mass of gall stones lodged in small intestine producing chronic inflammation and thickening of lower portion of ileum and cæcum, with ulceration of mucous membrane	Prior to recent illness had severe bilious attack and constipation subsequently chronic diarrhœa with constant vomiting before death	Ileum three inches from valve much contracted and mucous membrane entirely destroyed mass of gall stones two inches long and one and a quarter inch in diameter impacted three feet from cæcum gut ulcerated and thickened and strictured below it large gall-stones in ileum and cæcum in addition to the first mass
32	M	14	Ibid. vol iii p 366 Mr Beale	Diverticulum of ileum containing orange pips, cherry stone &c perforation from ulceration	Vomiting constipation eight days before death acute peritonitis subject to previous attacks of constipation.	Diverticulum about fifteen inches from cæcum of the same diameter as the intestine at its origin but increased considerably as it extended into pelvis at extremity at least twice as broad as at attachment about three and a half inches long At upper part its coats were thick, and resembled that of intestine but its lower half was thin and rotten and was readily torn while being examined.

We now wish to draw attention to the relative number of the causes of obstruction included in this list of cases, and for this purpose we divide them as we have already done those which Mr Cæsar Hawkins collected in his table *

		No of cases
1	From cancerous stricture	4
2	From non cancerous stricture	4
3	From stricture after hernia, 1	
4	From internal strangulations by bands, adhesions, &c	6
5	Displacements, twisting, and of sigmoid flexure	4
6	Foreign bodies or calculi lodged in intestine	4
7	Diverticulum containing foreign bodies	1
8	Intus susception	3
9	Fibrous polypi, associated with intus susception	2
10	Meso colic hernia	2
11	Obturator hernia	2
		32

In the investigation of such an important subject as that upon which we have now entered, one great difficulty is to offer sufficient evidence by illustration of cases, without tiring the patience of the reader, or rendering this article a tedious essay This difficulty we have experienced, for, in the analysis of such papers as we have undertaken, it is our bounden duty, if we accord merit to the authors of those writings, or venture to criticise their recorded opinions—it is our bounden duty, in justice to them and to ourselves, to prove by facts, rather than by theories, or by both, if in our power, that where we bestow praise, it is not from flattery—where we differ, it is not from prejudice

To test the value of these tables (that given by Mr Hawkins, and the one drawn up from the cases presented to the Pathological Society) by merely recording many other cases, would be a matter of no very great difficulty But the objection that weighs against such a proceeding, is, that there is wanting, in the record of many of these cases which have been published from time to time, that accuracy of detail respecting the post mortem conditions, which renders so many of them void, as evidence or a test, where accurate pathological observations are so essential an element in the sifting of evidence, to establish the relative, or even positive, occurrence of obstruction In this difficulty we have, though with some hesitation, determined to introduce, as a comparison to the summary of cases reported by Mr Hawkins and by the Pathological Society, a summary of the pathological specimens illustrating this subject in the collection of the Museum of the College of Surgeons Objection will, no doubt, be made to a collection in a museum being brought forward as evidence to test the accuracy of a statement respecting the *relative frequency* of a disease, or to prove the truth of any *statistics* of medicine or surgery, and in most diseases—we might say almost all—the objection would be held on right principles, but in a disease affecting a set of organs as the intestines, and which disease has, in most cases, so rapidly and invariably proved fatal in its result, a museum like that from which we have selected the following list, may surely be taken as evidence which furnishes something of a comparison, regarding the relative frequency of the causes of obstruction affecting the intestinal canal

---

* Some cases published in the Pathological Society s Transactions have been omitted, as they have already appeared in Mr Cæsar Hawkins' table of cases See Med Chir Trans , vol xxxv

From the collection in the Museum of the College we find the following causes of obstruction

		No of cases
1	From cancerous obstructions	14
2	From non cancerous strictures	16
3	From adhesions of intestines	3
4	From intus susception	8
5	From polypus, warty growths, or epithelial cancer associated with intus susception	3
6	From foreign bodies	3
7	From obturator hernia	1
		48

If we now take into consideration these three masses of evidence, they will at any rate furnish us with one hundred and twenty seven cases for analysis, and though, as far as symptoms are concerned, the specimens from the College are, in many instances, devoid of history, still with respect to the actual seat of disease, they offer much that is valuable for our notice

	No of cases.
From Mr Hawkins' table we have taken	47
From the Pathological Society's Transactions'	32
From the Museum of the College	48
	127

From these we have arranged the following table

		Mr Hawkins	Path Soc	Coll Mus	Total
1	Cancerous obstructions	20	4	14	38
2	Non cancerous stricture	21	4	16	41
3	Twists or displacements	1	4	0	5
4	Bands producing internal strangulation	1	6	3	10
5	Obstruction from external tumours or abscesses	2	0	0	2
6	Foreign bodies, calculi, &c	0	4	3	7
7	Diverticulum	0	1	0	1
8	Intus susception	0	3	8	11
9	Ditto, associated with polypi or other growths	0	2	3	5
10	Meso colic hernia	0	2	0	2
11	Obturator hernia	0	2	1	3
12	Unknown	2	0	0	2
					127

If we are permitted to draw any conclusions from the evidence now before us, the conclusions must necessarily be, that in a large majority of cases, especially in advanced life, the causes of obstruction will be dependent on stricture of the intestine—the result of cancer affecting its walls, or some other deposit or disease existing therein—producing thickening, gradual contraction or diminution of the diameter of the bowel, and ultimate and effectual impediment of the passage of its contents, if the disease is beyond the reach of local treatment

It is interesting, before we consider the effects of the various causes of obstructions, to note more particularly the relative situation of these strictures in the large and small intestine, as far as our cases enable us to do so, for in alluding to the question of treatment, we shall have again to refer to these points Separating the various specimens of stricture depending on an altered condition of the bowel itself from all other causes of obstruction, we observe them to have affected the intestine in the proportions noted in the following table

Portion of Intestine	Cases of Cancer					Cases Non cancerous				
	Mr H	P S	C M			Mr H	P S	C M		
Small intestine	0			8		0	1	1		
Large intestine colon	0			4		4	0	2		
Ileo cæcal valve & cæcum	1			1		0	0	1		
Sigmoid flexure	3			5	29*	11	1	*0	2	31*
Rectum	15			24		5	2	12	9	
Total of cancerous cases				37		Total non cancerous			40	

From this table we find that 77 cases of stricture of the intestine were observed in 127 cases of obstruction, and that in 60 of these the situation of the stricture was *below* the descending colon, either affecting the sigmoid flexure or the rectum

We have already observed that the cases we have selected should not be considered as pre eminent examples for the purposes of analysis or comparison, and we cannot but feel the force of an objection which might be urged against this latter table, as fallacious in affording any correct data with respect to the relative frequency of the occurrence and seat of stricture of the intestinal canal We allude to the fact of taking Mr Hawkins' table of cases, as furnishing any indications of the general causes of obstruction, when probably many or most of those cases were selected by the operators as favourable instances for treatment, in consequence of the cause of stricture being suspected or known to exist low down in the gut But upon a careful perusal of this table (i e Mr Hawkins'†) it is not presumptuous to conclude, that before very many of the operations were performed, great doubts existed as to the exact seat of the stricture, that the diagnosis was but in comparatively few cases accurate or certain, and that the cases were decided to be operated on, as often on account of the urgency and danger of the symptoms, as from the circumstance of the causes of that urgency and danger being positively ascertained At any rate, it is only by examining such accurately recorded and well authenticated cases that we shall be enabled to arrive at any established conclusions as to their condition and frequency, and if the tables now put forth should be objected to on account of the reasons we have mentioned, it will, we hope, be conceded, that though we have not quite arrived at the truth, and that therefore our conclusions are in this respect premature, yet that we have advanced towards an approximation of the truth, and that this investigation has, so far, not been unprofitable

Respecting the effects of obstructions, both locally and generally, there is a great deal that is interesting to reward us for our enquiry, and much that is valuable to repay us for an investigation Careful enquiry may elicit something that should lead to beneficial practical results Minute investigation must yield more towards the elucidation of its numerous pathological obscurities

---

* Total cases *below* descending colon

† Med Chir Transactions, vol xxxv

Mr Cæsar Hawkins has so clearly noted for us the causes of death in 30 out of 43 cases mentioned in his first table, that we cannot do better than transfer the statement in his own language to these sheets He says—

"Of the 21 cases which did not recover from the operation [for artificial anus] the assigned causes of death were—

"In one case, two pounds of mercury given previously, dragging the ileum into the pelvis

"In one case, unrelieved, the obstruction being above the opening

"In one case, fæcal evacuations incomplete

"In five cases, exhausted by the disease

- *a* Cæcum burst and fæces escaped into pelvis
- *b* Ulceration of bowel
- *c* Rupture of six inches of peritoneal coat from distension

"In seven cases, peritonitis

- *a* Old, as well as recent
- *b* From the operation
- *c* Begun before operation
- *d* From cancerous ulcer

"In one case, unknown, but cancerous

"In one case, chiefly sloughing of sacrum

"The deaths of nine patients who recovered from the operation have been recorded, of which—

"Four cases were cancerous one died of phthisis, one of the disease and dropsy, and two of the disease

"Five cases were stricture of the colon and rectum not cancerous"

In the appendix added to the table there are four more cases mentioned, all of which are dead, one died unrelieved by the operation, one had rupture of the peritoneal coat in three or four places, one died of sloughing of the wound, and one apparently worn out by the disease nearly five months after the operation It is probable that at this present time only two of the cases tabulated by Mr Hawkins are alive

In taking a hasty view of the effects related to have occurred in the cases tabulated from the 'Pathological Transactions,' we observe that

In four cases of simple stricture (Nos 1, 17, 22, 24,) two cases presented evidence of peritonitis the first after nineteen days' constipation, the second after three weeks' constipation, and in the latter gangrene of the mucous membrane had commenced, the third case sank after nine days' constipation, the fourth died from the effects of long-continued obstruction, though to the last the obstruction was not complete

In four cases of cancerous stricture, all of the larger intestines, one died of peritonitis, history not complete, in the second a spontaneous communication externally occurred some time before death, above the stricture, the last two sank exhausted, one after sixteen days' constipation

In ten cases of obstruction resulting from internal strangulation, obturator and meso colic hernia, four died, either from peritonitis or gangrenous intestine, one from perforation of the intestine, though in one case of constipation of fifteen days, and in another of three weeks, there were no marked symptoms of peritonitis

In four cases of twists &c of intestine, there were indications of peritonitis, or aggravated mischief in the bowel itself, six or seven days appear the longest period of constipation in these cases

In three cases of intus-susception, the bowel was seriously affected by inflammation in two of them, they all terminated fatally within a few days, four days being the longest period of duration mentioned

In two cases of fibrous polypi complicated with intus-susception, one had gangrene of the intestine, the other extensive ulceration of the bowel and peritonitis

In five cases of foreign bodies lodged in the bowel, one had perforation of the intestine, one sank after long-continued irritation and ulceration of the bowel, two had peritonitis, and one sank under sudden collapse

The longest period of constipation noted amongst these cases was that of three weeks, occurring in a case of obturator hernia (No 23), in which no active symptoms of peritonitis existed The shortest period in which death occurred after symptoms of obstruction were noted, was forty one hours, in a case of internal strangulation The length of time that constipation may persist, unattended with any serious local mischief, is a matter as surprising as it is interesting, with respect to the consideration of our remedies in such cases It is almost incredible to what an extent the bowels can adapt themselves to their daily increasing contents, or accommodate themselves to difficulties which are imposed upon them by degrees, not hastily, or continued for too long a period

In two cases in Mr Hawkins' table, it is stated that constipation had endured for forty days, operation for artifical anus was performed, and as regards the immediate condition of the patients, they recovered In another case in which constipation had existed for some forty-five or fifty days, the patient survived the operation ten days In several other cases constipation had continued thirty or more days It has occurred to our own knowledge that constipation continued for two months in a case of partial stricture of the rectum, and from this the patient recovered, the stricture now freely allowing the passage of fluid fæces Dr Burne quotes an instance of constipation of still longer duration, though not dependent on stricture, and from which the person recovered

Must constipation always be present in order to establish the existence of obstruction? Should we invariably consider its absence conclusive evidence as contra-indicating the *possibility* of obstruction? Let us look for an answer among the cases furnished from the Pathological Society

In Case 8, a twist of the intestine, the constipation was not complete, the symptoms in the first instance were those of diarrhœa In Case 12, cancerous stricture of the transverse colon, constipation was not complete, and in Case 13, colloid cancer of the rectum, there was sometimes diarrhœa

In Case 24, stricture of the ileum following an operation for strangulated hernia, the constipation never appeared to be complete, *but the intestine became gradually distended* In Case 27, strangulation of the ileum, the constipation was not quite complete, though great obstruction existed for five weeks In Case 31, gall stones impacted in the intestine, the latter being ulcerated and contracted, there was diarrhœa up to the period of death, though in the primary symptoms constipation was well marked for some days We shall have again to refer more minutely to the different causes producing these variations in the symptoms—that is, with respect to the action of the bowels But it will be familiar to our readers to have observed the alternations that may occur, especially in cancerous obstructions, between constipation and purging, from the ulceration of the parts, or the breaking-down of the soft masses which sometimes project into the cavity of the intestine, and in the management of those cases of cancerous stricture within reach of the finger, Mr Hawkins illustrates the advantage of bearing this circumstance in mind He was consulted in the case of a lady labouring under six days' obstruction of the bowels, whom he had seen frequently on previous occasions, believing her disease to be cancerous For some time before the six days' obstruction, numerous abscesses and sinuses had formed, and the whole of the fæces had been for some weeks discharged by a large ulceration into the vagina Mr Hawkins, with some difficulty, broke through the obliterated anus, and nearly five inches of the rectum filled with the morbid growth, and some portion beyond the reach of the finger, through which he could not force a bougie, gave way four hours afterwards, so as to empty the distended bowel Notwithstanding this extensive disease, the health of the lady was, after three years' duration, perfectly unaffected, and the bowels continued more amenable to medicine than before Mr Hawkins employed these measures

Constipation cannot, therefore, be always considered as essential to the existence of obstruction Must it not, however, be always essential to the proposal for any surgical interference? We think, even on this point we may hesitate to give unconditional consent, and it will be our endeavour to argue the question from the facts already before us, when we have to speak upon remedial measures As constipation varies in its degrees, it cannot be regarded as an invariably attendant symptom in all cases of obstruction, but it must still be considered as one of the most important and one of the least capricious symptoms for the guidance of our diagnosis

Accuracy of diagnosis with respect to the seat of obstruction and its cause can perhaps never be arrived at with any degree of certainty in the large majority of cases, in some, however, the evidence is sufficient to enable us to determine not only the cause and its situation, but the remedy best adapted to the urgency of the symptoms

Mr Phillips observes, in reference to the difficulty of diagnosis from general symptoms, and after relating the particulars of several cases,—

> "The impression which must be produced by reading the preceding cases is this, that, no matter what may be the cause of the obstruction, no certain remarkable difference is observed in the more prominent symptoms by which it is accompanied There are, in all, abdominal pain, abdominal tension, obstinate constipation, and sickness, but in the mode of their occurrence it would be difficult to point out any distinct difference Thus constipation, abdominal pain or uneasiness, with

tension and sickness of the stomach, are present, in greater or less variety, in most cases but there is no certain or definite line to be drawn, either as to the time they occur, or the severity with which they are ushered in, whether the obstacle be a biliary calculus or a band of false membrane"

He then goes on to say

"Taking the symptoms altogether, it is true, when the obstruction is complete and suddenly developed, they are usually more urgent than when it is slowly produced, but there are cases in which the obstruction has terminated life within forty eight hours, without much sickness or pain In cases of invagination, or where a tumour gradually encroaches upon the calibre of the tube, where the canal becomes contracted by change of structure, the pain does not commonly, even up to a late period, exhibit so much intensity as in the former case But I say again, that we cannot with confidence rely upon these distinctions, the exceptions are so many"

There is much that is too true in this; the history and post-mortem conditions of the cases in our table confirm in a great measure the correctness of the conclusions at which Mr Phillips has arrived For it will be observed that constipation was not a symptom attendant on all the cases of obstruction, diarrhœa even existed in one of them till the time of death, and in one case related by Mr Shaw, there was such a collection of fæculent matter contained in the intestine beyond the seat of the stricture, that a copious evacuation *might* have occurred, and yet any favourable expectations that would have been formed from such an event, in treatment, would ultimately have ended in disappointment

The cases in which we may, to a great extent, venture a tolerably positive diagnosis, are the cases in which obstructions are situated in the large intestine, and more particularly its lower portions, and fortunate indeed does it appear, as the table drawn up by Mr Cæsar Hawkins fully establishes, that these are the cases in which our remedies are most readily applicable and most favourably applied—not from the greater facility of the diagnosis, but rather from the situation of the strictured portion of intestine, and these remedies appear likely to be attended with more satisfactory results and greater success than any attempts we may make to relieve the effects of obstruction from other causes, or elsewhere situated

Our space will not now permit such a full consideration of the various points relating to intestinal obstructions as we could desire, we have already been compelled to omit something—we are obliged to defer much to another opportunity, when that opportunity offers, we shall resume the subject of their diagnosis, their treatment, and their pathology

George Pollock

REVIEW III

A Treatise on Percussion and Auscultation. By Dr JOSEPH SKODA. Translated from the fourth edition by W O MARKHAM, M D, Assistant-Physician to St Mary's Hospital —*London* 8vo, pp. 346

DR. MARKHAM has rendered essential service to the medical profession in England by his translation of Skoda's work The public mind in this country has already been well prepared by Dr Herbert Davies' lectures, and by various papers and extracts that have appeared from time to time,* but it is always satisfactory to read an author's own explanation of his peculiar views, and the reasons that have induced him to differ from his predecessors Dr Markham's translation is in general literal, and yet elegant and readable we have noticed but few inaccuracies in it In his preface he gives a short summary of Skoda's leading doctrines Before proceeding to examine the work before us in detail, it may interest the reader if we premise a very few observations on Skoda's method of clinical observation, and on his merits as a practical observer

Skoda has for many years been clinical professor at the general hospital at Vienna, an establishment containing about 2500 beds His clinical wards, though only containing about forty patients, are always full of instructive cases, as the clinical professor has the right of first selection from the new patients, who are admitted into the hospital The cases in his wards are mostly acute, or are grave organic lesions, the mortality is consequently high, and the opportunities of verifying the diagnosis during life by post mortem examination, frequent

Skoda comes every day at about 8 A M, and examines the patients in the presence of his class, making any observations which he may think instructive New cases are taken by the students, and the report is read over at the bedside Skoda makes observations on the report, and then examines the patient himself, lecturing upon the case as he proceeds, and pointing out his reasons for coming to his conclusions The diagnosis is sometimes not completely settled the first day, nor until he has seen the patient two or three times—but when made, is recorded with great minuteness, and is scarcely ever changed If the patient die, the post-mortem examination is made by Rokitansky, or his assistant, and the result recorded without reference to the report of the symptoms or diagnosis during life The two statements of the physical lesions, the one made before, the other after death, are then read out to the students attending the clinical wards, and if any difference exists, the causes of the false diagnosis, and the mode of avoiding such an error, if possible, in future, are pointed out

Sometimes the diagnosis is doubtful—i e, two diseases are named, either of which might have produced the signs present, but the probability may be in favour of one rather than of the other But in the majority of cases the diagnosis is precise, and when once given in a thoracic disease, we have not seen more than one or two mistakes during four months' attendance at the dead-house and in the clinical wards The accuracy and certainty of Skoda's diagnosis appeared to us to be very great, and we believe that most of those who have had the advantage of watching Skoda's practice for any length of time, will agree with us in this opinion

---

* In the Edinburgh Medical and Surgical Journal for 1841, there is an analytical account of Skoda's work by Dr Drysdale and Dr Russell, which however is too short and defective to give an adequate idea of the changes introduced by Skoda.

A method by which such accurate results can be arrived at, deserves our most attentive consideration, and cannot be passed over by treating it as German mysticism Added to this, Skoda's work has now become the text book in Percussion and Auscultation at most of the German universities, and its great value is admitted in Germany by all those best qualified to judge of its merits

The tendency of Skoda is to simplify instead of to complicate the subject of which he treats, though possibly he has himself fallen into one or two errors, from not accurately perceiving the essential conditions upon which some of the sounds he describes depend We here allude more particularly to the distinction he makes between the "full" and "empty" percussion-sounds We also agree with Dr Markham, that his nomenclature would have been better had the theory of consonance not been introduced into it

> "The introduction into our language of a plain, practical nomenclature of auscultation is much required, and would be a great boon conferred upon the science Skoda's nomenclature is eminently plain and to the purpose, but unfortunately is in part based upon his theory of consonance he speaks, for instance, of consonating rales, so that its assumption can, I fear, hardly be hoped for, until such time, at least, as his theory has met with more general approbation" (Markham, preface, p 11)

Skoda's theory of consonance is well summed-up by Dr Markham in his preface, but as we have on a former occasion referred to this subject in a review of Dr Herbert Davies's work, it is unnecessary to return to it on the present occasion

The first part of the work before us treats of the phenomena observable by the aid of percussion and auscultation Skoda considers that one percussion-sound may differ from another in one or more particulars

1st It may be full or empty
2nd It may be clear or dull
3rd It may be tympanitic or non tympanitic
4th It may be high or low

1*st Class* —"A full percussion-sound may be clear or dull, tympanitic or non-tympanitic, high or low, and the same is true as regards an empty sound"

The difference between full and empty is thus described

> "We do not judge of the size of a resonant body by the strength of the sound which strikes upon the ear the slightest vibration of a large bell tells us of its magnitude, the loudest ringing of a little bell misleads no one as to its smallness, neither do we judge of the dimensions of bodies from the pitch of their sounds There is no good general term to designate that quality of sounds which characterizes the size of bodies I believe that in singing, and instrumental music, the word *full*, or full toned, or sonorous, is used, I shall therefore borrow the expression in speaking of the percussion sound When any one percusses with equal force different parts of the thorax and abdomen, he will find that in some places the

sound appears more persistent, and, as it were, spread over a larger surface than it does in others the first kind of sound I call full, the second, the less full, or empty percussion sound" (Markham, p 8)

This distinction between a full and an empty sound is, we believe, more difficult to catch than anything else in the practice of percussion, and this is partly due to a complication unnecessarily introduced by Skoda One sound can only differ from another in one of four particulars—

1st A difference in loudness
2nd. A difference in duration
3rd A difference in pitch
4th A difference in timbre, by which we mean, that character of a sound which enables us to determine the instrument whence it proceeds thus, if the same note be sounded on the horn and on the violin, we at once know from which it has proceeded, and the character which enables us to form this judgment, is the *timbre* of the sound

Beyond these four differences, it is not conceivable that any distinction can exist What, then, becomes of Skoda's *full* and *empty* sounds?

To take Skoda's own example What are the peculiarities in the sound of a large bell, that tell us of its size? They are, 1st, the longer *duration* of the sound than that of a small bell, and this difference Skoda seems to have perceived 2nd, the deeper note of a large than of a small bell, although Skoda himself does not allow that the *pitch* affects its fulness or emptiness

In our opinion, then, Skoda's full sound is one which is long in duration, and at the same time *deep* in pitch, while his empty sound is one which is shorter in duration and *higher* in pitch Dr Traube of Berlin, whose skill in percussion and auscultation we have had frequent opportunities of observing discards altogether Skoda's *full* and *empty* sounds, substituting in their stead the words *deep* and *high*, and this nomenclature has the advantage of being precise and intelligible

A deep percussion sound is almost always longer in duration than a high sound, so that for practical purposes the duration may be disregarded, and the pitch alone attended to

If the meaning above attached to the words *full* and *empty* is borne in mind, the conclusions arrived at by Skoda are not interfered with, and greater precision is obtained

2nd Class—Clear and *dull* Skoda does not attempt to define these terms, but uses them in their commonly received meaning, yet we think that much is to be gained by accurately finding the meaning of the terms we employ, and we would suggest, notwithstanding the mystery which has often been attached to these differences, that the only real distinction between a clear and a dull sound is, that the former is louder, and of longer duration than the latter

Differences in pitch have, in our opinion, nothing to do with the clearness or dulness of a sound, but have reference merely to its being full or empty A completely dull sound is, at the same time, a completely empty sound, as in both the duration is reduced to a minimum

3rd Class—Tympanitic and *non-tympanitic* The word *tympanitic*

(*Tympanitische Percussions-schall*) is open to some objection, as it is often employed in common language to denote any loud, deep sound produced by percussing over a large air-containing cavity. What Skoda means by it, may, however, be readily understood by percussing the healthy abdomen. The abdomen (unless enormously distended with air) always yields a tympanitic sound.

> "The non tympanitic sound is represented by the sound which percussion produces at those parts of the thorax beneath which lies healthy lung, normally distended with air. An abnormally distended lung, as in vesicular emphysema, gives at one time a tympanitic, at another a non tympanitic sound. A partial emphysema in the midst of lung deprived of air (as happens in pneumonia, where not unfrequently the tissue around the hepatized portion, and especially at the borders of the lung, is emphysematous), generally produces a tympanitic sound, but if the whole of the lung is emphysematous, the sound is seldom distinctly tympanitic.
>
> "When the lung is much reduced in volume by compression, but still contains air, its sound is invariably tympanitic." (p. 13.)
>
> "In pneumothorax, the walls of the thorax, if they are not much distended, yield a tympanitic sound, but if much distended, their sound is almost constantly non tympanitic." (Markham, p. 15.)

Skoda disputes the accuracy of Dr Williams' explanation of the cause of the tympanitic percussion sound, occurring at the apex of the lungs in some cases of pneumonia and pleurisy, and our observations lead us to concur in Skoda's explanation, for we have more than once heard the sound in question, where the subjacent lung was proved by post-mortem examination to be perfectly pervious, and consequently not in a condition to conduct the tracheal percussion-sound with increased intensity.

Skoda believes that the tension of the walls of the air-containing cavity diminishes the tympanitic percussion sound, while their flaccidity favours its production. One of his experiments is very simple and conclusive. Take a stomach, one of whose apertures has been tied, gradually inflate it, and percuss it in different stages of inflation. When its distension is moderate, the percussion sound is tympanitic, but as soon as the walls become very tense, the tympanitic character of the sound ceases, and at the same time, the sound becomes more dull.

The *4th class* of percussion-sounds (high and low) needs no further discussion.

Having disposed of the subject of *Percussion*, Skoda passes on to *Auscultation*. We shall select some points here and there for consideration.

Pectoriloquy is a sign which he discards as being undistinguishable from Bronchophony, and he affirms, what is indeed now admitted by many, that Laennec's pectoriloquy may exist over hepatized lung without a cavity, and, on the other hand, that a cavity may exist without pectoriloquy. To this point we have also formerly alluded, in reviewing the works of Drs. Walshe and Davies.

Laennec's *ægophony*, as pathognomonic of pleuritic effusions, Skoda would also discard, as he affirms that he has often heard it where no effusion existed, for example, in cases of pneumonia and in tubercular infiltration, with or without cavities. This statement is also now received by many able auscultators. Skoda's own division of the modifications of the thoracic voice is not, however, characterized by his usual simplicity He says—

"I distinguish the following modifications of the thoracic voice

"1 The voice, accompanied by a concussion in the ear, completely traverses the stethoscope—*loud* bronchophony, which may be either clear or dull

"2 The voice, unaccompanied by concussion in the ear, passing incompletely through the stethoscope—*weak* bronchophony

"3 An indistinct humming (*Summen*), with or without a barely appreciable concussion in the ear

"4 Amphoric resonance, and the metallic echo of the voice" (Markham, p 75)

As the first two classes of bronchophony are merely differences in degree, it is not safe to form a diagnosis from the presence of one or other of them, for what one person would call strong, another might call weak bronchophony, so that a want of precision is likely to arise from this division, similar to that arising from Laennec's separation of pectoriloquy and bronchophony

The *murmurs heard during respiration* are defined by the character of the sound produced in imitating them by the mouth Thus the murmur of the air in the larynx, trachea, and large bronchi, may be imitated by pronouncing *ch* guttural (as in the Scotch *loch*), or it falls between *h* and *ch*. If, while in this position of the tongue and soft palate, the air is drawn in and out, a murmur much resembling that of the larynx or trachea will be produced The *vesicular* murmur

—"may be imitated by narrowing the opening of the mouth, and then drawing in the air The consonant of this murmur falls between *v* and *b* This character, however, refers only to the murmur of inspiration, that of expiration, in the natural state of the respiratory organs, causes little or no sound in the air cells and finer bronchial tubes, whatever sound is heard, differs from the murmur of inspiration, and resembles rather a gentle aspiration (*Hauchen*), or blowing (*Blasen*) It can only be imitated by the mouth during expiration, the consonant which represents it falls between *f* and *h*" (p 86)

The pitch of the murmur is expressed by the vowel, which must be combined with the consonant, to imitate it by the mouth—*e* corresponding to the highest note, and *u* (or rather *oo*, as in good) to the lowest

This accurate determination of the consonant and vowel required for the imitation of any particular sound has advantages besides the increased precision of description which it allows The student not very familiar with the practice of auscultation, will find, that it will greatly assist his recognition of a doubtful sound, if, while listening, he imitates the sound with his mouth, and thus determines the consonant and vowel belonging to it

Having defined the terms vesicular and bronchial breathing, Skoda considers all intermediate sounds as *indeterminate* (*unbestimmt*), i e, if they are not distinctly vesicular, or distinctly bronchial in character, he draws no conclusion from them, for such indeterminate breathing may exist over perfectly healthy lung, or over lung in various degrees of consolidation

Laennec's "*cavernous breathing*" is discarded for the same reasons as pectoriloquy and gurgling—viz, because it occurs, and that very frequently, over simply consolidated lung, and is undistinguishable from bronchial breathing This must of course be understood only of Laennec's cavernous breathing, not accompanied by amphoric echo, to which Skoda attaches the same importance as Laennec, and considers it to be pathognomonic of a large cavity

The râles are enumerated as follow

1st The *Vesicular Râle* is characterized by the small and equal size of the bubbles, and is produced by the entrance of air into the smallest bronchial tubes and air-cells, in which blood, mucus, serum, &c, exist

2nd The *Consonating Râle,* corresponding in pathological meaning to bronchial breathing, and marked by its clearness and its high pitch The bubbles are of unequal size

3rd The "*dry Crepitating Râle* with great bubbles," as described by Laennec

4th The *Indeterminate Râles*

> "Under this head I comprise all rales which are neither vesicular nor consonating, and are not accompanied by amphoric resonance or metallic tinkling, they offer no special indications respecting the condition of the lung tissue" (p 131)

Sonorous whistling and hissing sounds are distinguished from the râles, they may or may not consonate

Amphoric echo and metallic tinkling are next discussed Of the relation of these sounds to each other, Skoda says

> "Any one may convince himself, by the production of metallic echo in a room, and by directing his voice into a vessel, that amphoric echo and metallic tinkling are phenomena which occur under similar conditions, and that metallic tinkling bears the same relation to the amphoric resonance, as does a high to a deep flageolet tone of a guitar string" (Markham, p 133)

He then proceeds to show, that for the production of metallic tinkling, fluid is not necessary, and for that of amphoric resonance of the voice and breathing, it is not necessary that a communication should exist between the cavity and the bronchi, which indeed is very rarely persistent in cases of pneumothorax

> "If a person speak into a stethoscope, placed on a stomach filled with air, both metallic tinkling and amphoric echo will be heard sounding within the stomach, and this, whether the stomach be partially filled with, or contain not a drop of fluid" (p 134)

This experiment illustrates both the foregoing propositions Skoda does not by this mean to deny that the falling of drops of liquid in a large cavity will produce metallic tinkling, but only asserts that other sounds, such as crepitations &c in the neighbourhood, may also cause it

It will be seen that *gurgling* is not a sound recognised by Skoda Laennec's gurgling is what Skoda calls a consonating râle with large bubbles, and this is not a mere change of name Laennec affirmed that *gurgling* is pathognomonic of a cavity, Skoda, that it may occur over consolidated lung, where there is no cavity, and we think few physicians of experience will deny the frequency of this occurrence But gurgling is, in the minds of many, so associated with the idea of a cavity, that if by other signs they determine that no cavity exists, then the sound, which a minute before was *gurgling,* now is called "large crepitation approaching to gurgling," yet the sound remains the same, and had there not been signs present to disprove the existence of a cavity, it would have passed for *true gurgling,* and a cavity would have been diagnosed We have seen very experienced auscultators fall into this mistake, which might always have been avoided, by recollecting that the sound in question may proceed from fluid contained in the tubes of consolidated lung, as well as from a cavity Pectoriloquy is of no assistance to us here, for this sound, as Skoda has shown, has the same signification as, and passes by imperceptible shades into, bronchophony Of course, if these sounds occur at the apices of the lungs in a chronic disease, with profuse expectoration, where there has been hæmoptysis, &c, the probability is, that they arise in cavities, while if they occur in the course of pneumonia, the probability is, that they merely denote consolidation of the lung

The chapter on the *pleural friction-sounds* need not delay us, except to notice that Skoda does not sufficiently discuss the great difficulty there often is in distinguishing the finer crackling friction-sound from the crepitating or crackling râles

Chapter IV treats of the auscultatory phenomena presented by the organs of circulation, that have been brought forward in explanation of the heart's impulse, which he himself attributes to the following causes

1st The blood propelled from the ventricles into the large arteries, and especially from the left ventricle into the aorta, presses against the walls of the ventricle with a force that may be estimated by its velocity in the artery, and this force is relatively exerted in a greater degree at the apex—viz, the part opposite to the aorta and pulmonary opening This is the recoil theory of Dr Gutbrod

2nd Lengthening of the ascending aorta, allowing the descent of the heart during systole

3rd "When the heart is enormously enlarged," says Skoda, "I believe that it may raise the walls of the thorax in consequence of its antero posterior diameter becoming greater during the systole, than the space between the fore part of the thorax and the vertebral column, *from which it must be admitted*, that the heaving of the thoracic walls, when no sign is present to demonstrate a simultaneous and equal pressure on the vertebral column, is only explicable by Dr Gutbrod s theory" (Markham, p 160)

This does not make sense There is here an error in the translation, the words printed in italics are in the German, "wobei ubrigens zugestanden werden muss," which ought to have been translated, "*although at the same time it must be allowed*," &c, that is to say, as under such circumstances the vertebral column must be pressed upon equally with the anterior wall of the thorax If it be not so pressed upon in any case, Dr Gutbrod's theory becomes the only conceivable one

4th "Lastly, it must not be forgotten that the heart, by becoming rigid from contraction during its systole, takes a different *position* and direction from those it had in its relaxed state, and that such a change of position may aid in producing its impulse, even though the idea of a lever like elevation of its apex against the thoracic walls be hardly explicable by the arrangement of its muscular fibres (Skoda, p 154)

Dr Markham has here also made an oversight in his translation, the word *lage* (*position*) he has rendered by *form*, which makes the meaning obscure

After a critical examination of some of the leading theories of the *sounds* of the heart, Skoda prefaces his own view (which we shall not discuss) by a very elaborate description of the mechanism of the valves He adopts Weber's explanation of the cause of the insertion of the tendinous cords, of the auriculo-ventricular valves, into papillary muscles, and not directly into the walls of the ventricles

> "The area of the cavity of the ventricle at the commencement is different from its area at the end of the systole, and the points of origin of the papillary muscles approach constantly nearer to the attached border of the mitral and tricuspid valves, as the systole progresses Now the length of the tendinous cords always remaining the same, it becomes evident, that for the perfect closure of the valves, the tendinous cords, which retain the valves in their proper position, must of necessity take their origin from the papillary muscles If, for example, they arose immediately from the walls of the heart, and were of due length for the performance of their functions at the commencement of the systole, they would necessarily be of too great a length during its progress, and, on the other hand, would impede the diastole, if their length was sufficient merely to retain the valves in their proper position towards the end of the systole But as the length of these cords is invariable, they must necessarily have some muscular attachment, and the object of the papillary muscles is evidently, by alternate extension and contraction, to retain the valves in their proper position During the progress of the systole these muscles contract, the distance between their points of origin and the attachment of the mitral and tricuspid valves is constantly diminishing, in consequence of this contraction, the tendinous cords arising from the muscles retain the same degree of tension during the progress of systole, which they had at its commencement and they also retain it during the diastole, the papillary muscles being lengthened, while the heart expands The correctness of the views here offered of the functions of the papillary muscles seems to be confirmed by the fact, that that division of the tricuspid valves which lies against the heart s septum, receives its tendinous cords either from very short papillary muscles, or immediately from the walls of the heart The points of origin of these tendinous cords approach little, or not at all, during the systole, to the points of attachment of the valve and recede as little during the diastole Now, here a simple tendinous cord suffices for the proper restraint of the valve, because change in the length of the cord is not required" (Markham, pp 190, 191)

Skoda lays great stress on the importance, as a diagnostic sign, of a strengthening of the second sound of the pulmonary artery, as compared with that of the aortic valves The former is to be listened for between the second and third ribs to the left of the sternum, the latter in the corresponding position to the right of that bone In this position we auscultate, not over the respective valves, but in the course of the large vessels, since the aorta inclines to the right, and the pulmonary artery to the left Experience shows, that in these two positions the second sound of the heart is of different intensity in some diseases An increase in the intensity of the sound of the pulmonary artery proves increase of pressure in the pulmonary artery, which, if not due to obstruction in the lungs, and if the patient has no fever, almost always proves, according to Skoda, obstructive or regurgitant disease of the mitral valve It is easy to see how such a disease will increase the pressure in the system of pulmonary veins and arteries, and to this cause is soon added the compensating hypertrophy of the right ventricle Skoda does not think that we are warranted in diagnosing regurgitant disease of the mitral valve from a systolic murmur loudest at the apex, unless at the same time the strengthening of the second sound of the pulmonary artery shows an increase of pressure in this vessel We refrain from discussing the correctness of this opinion

Part II treats of the physical signs as diagnostic of a healthy condition or of various morbid states of the thoracic organs Chapter I treats of these organs in their normal condition

In Chapter II, he proceeds to notice those deviations from the normal physical signs which take place in different diseases We can here do no more than notice, very briefly, a few of the points which are not so generally recognised in this country as they deserve to be

When pneumonia is invading a portion of lung, and before infiltration is complete, a tympanitic percussion-sound often appears over the affected portion, which is often the earliest physical sign of the impending mischief This modification of the percussion-sound can, of course, only lead to a conclusion, when from the other signs we have reason to suspect incipient pneumonia We have already noticed the reasons which make Skoda dissent from the opinion of Dr Williams as to the cause of this modification

With reference to the possibility of diagnosing by physical signs between red and grey hepatization, Skoda says—

> "I have frequently examined patients suffering from pneumonia, in whose lungs newly formed abscesses were found after death, but I have never in any single instance recognised the presence of abscess by the aid of auscultation and percussion In every case, the abscess, though communicating with the bronchial tubes, was filled with pus or sanies If the abscess be accidentally emptied of its contents, it will be rapidly filled again by the secretion, unless it be of long standing, and its walls have become solid, and their secreting power diminished" (Markham, p 277)

Laennec considered the "râle crepitant" to be pathognomonic of the first stage of pneumonia Skoda allows that it is sometimes heard in this stage, but asserts that he has often met with cases, in which, though he has watched them from the commencement, no crepitating rale has occurred, and he goes on to say—"It would be hard to say whether the râle, which is heard previous to the hepatization, or the consonating râle attendant upon complete hepatization, has been most frequently taken for Laennec's crepitating râle" The consonating râle which Skoda here mentions, differs from Laennec's *râle crepitant* by having the bubbles of which it is composed of unequal size, while in the *râle crepitant* they are equal, and exceedingly small

When speaking of the signs of vesicular emphysema, Skoda says—

> "Vesicular emphysema does not render the percussion sound tympanitic, unless the more fully inflated portion of lung is bounded by another portion of lung that is entirely deprived of air, which not unfrequently happens in hepatization and tubercular infiltration, or unless the emphysematous lung has completely lost its contractile power" (Markham, p 295)

With this observation we fully concur, it being remembered that Skoda's tympanitic percussion-sound, is that character of sound which is obtained by percussing the healthy abdomen

Solitary tubercles, even when developed in great numbers, do not, according to Skoda, produce any change in the percussion-sound, as long as the intervening lung tissue is healthy, and it is useful to bear this in mind, as tending to increase our caution in giving an opinion of the absence of tubercles, from the absence of changes in the percussion-sound Where, however, the deposit of solitary tubercles is confined to one portion of the lung, the percussion-sound will, according to our experience, be raised in pitch over this part, but when solitary tubercles are equally distributed throughout the whole of the lungs, even if they did raise the pitch of the percussion-sound, this change could not be appreciated, from the absence of any standard of comparison

It is often stated, that a partial pleuritic effusion may be distinguished from partial pneumonia, by causing the patient to change his posture, when the line of dulness is said to change, if the dulness be due to pleurisy This diagnostic sign Skoda considers of very slight value, as this change of position of the fluid in pleurisy very rarely occurs, and a little consideration of the pathological changes in the parts will show the reason of this permanence in the position of the fluid, for the fluid-effusion is almost always bounded by an exudation of albumino-fibrin, glueing the pleural surfaces together, so that almost every case of acute pleurisy is sacculated In partial *hydrothorax*, on the contrary, where the fluid is of the nature of a dropsical effusion, no such limitation takes place, and the fluid does change its position with alterations in the posture of the patient, though this change of position of the fluid requires some time for its occurrence

As a sample of the manner in which our author sums up the principal diagnostic signs in cardiac diseases, we shall give those of contracted mitral orifice

> "*Contraction of the Mitral Orifice.*—As in insufficiency of the mitral valve, the dulness of the percussion sound is increased in extent *in a direction corresponding to the breadth of the heart*, for in contraction of the left ostium venosum, hypertrophy of the *right ventricle* ensues still more rapidly
>
> "The second sound in the left ventricle is replaced by a murmur, which is often so prolonged as to be only momentarily interrupted during the heart's systole The second sound of the pulmonary artery is strengthened During the systole, either no sound at all is heard in the left ventricle, or only an *indistinct* sound, or again a murmur may be audible, because deficiency of the mitral valve is generally associated with contraction of the mitral orifice As a rule, the sounds of the aorta are weak, and the heart's impulse increased, and perceptible over a greater extent than natural" (Skoda, p 312)

Dr Markham has, in his translation of this passage, not faithfully rendered the meaning of the original On comparing our translation with his, it will be seen that he says, "the dulness is increased," without translating "der Breite des Herzens entsprechend" *in a direction corresponding to the breadth of the heart*, and instead of specifying that the increased dulness is owing to enlargement of the *right ventricle*, Dr Markham says, "hypertrophy of the *heart* occurs more rapidly"

Dr Markham has perhaps made these omissions with the intention of being concise, but by this abridgment, part of Skoda's meaning has been lost, and, in addition, an inaccuracy is introduced, for contraction of the mitral-orifice, though it is necessarily followed by hypertrophy of the right ventricle, does not give rise to hypertrophy of the whole heart The left ventricle, indeed, may be atrophied Another mistake in this passage is, that the word *indistinct* (*undeutlich*) has been by Dr Markham translated by *indeterminate*, which has quite another meaning in the writings of Skoda, and for which the German word "*unbestimmt*" is always used But some slight defects do not detract from the general merit of

Dr Markham's translation, which, taken as a whole, is faithful, and an acquisition to our medical literature In conclusion, we may remark, that though there are some parts of Skoda's work which, we think, might be improved, and though the controversial parts are sometimes rather longer than will suit the generality of readers, yet we can recommend the work before us, as well worthy of an attentive perusal, and we think its general circulation in this country will materially advance the science of percussion and auscultation

T R Armitage

Review IV

Der Nervus Phrenicus des Menschen Eine Monographie Von Dr Hubert Luschka, ausserordentlichen Professor der Medizin an der Universitat Tubingen

A Monograph on the Phrenic Nerve By Dr Hubert Luschka —*Tubingen,* 1853

Our present knowledge of the anatomy and the functions of the phrenic nerve might be nearly summed-up in the following words—viz, that it is distributed to the diaphragm, and that the movements of this muscle are interfered with when the phrenic is injured, or are arrested when the spinal cord is destroyed above the origin of the nerve The deficiency of knowledge concerning this nerve may undoubtedly be traced to the want of precision amongst anatomists, as shown, for instance, in the work of one of the most recent authorities on the nervous system (Valentin), in which the most rash, and, as Dr Luschka shows, inaccurate statements respecting its distribution are contained As long as this uncertainty in anatomical detail continued, it was not surprising that the true interpretation of actions should remain ungiven. It is therefore with pleasure we have perused a production in which fact is clearly distinguished from imaginings

The task that the author proposes to himself is, to prove the twofold function of the nerve, as in an ordinary spinal branch, to indicate the place of destination of its offsets, and lastly, to attempt to show its influence on parts in health, as well as to connect certain symptoms in disease with its deranged function. These several points will be referred to in the following pages, though our notice must of necessity resemble more an abridgment than a critical examination of the views propounded, for without following the author with the scalpel and the microscope, and undergoing nearly the same amount of labour, we cannot be in a position to impugn the accuracy of his statements But Dr Luschka's inquiries are too well known to need any recommendation at our hands, though, if the opinion of the reviewer may have any influence in securing an attentive perusal, it may be stated as his conviction, that our acquaintance with the anatomy, if not with the function of the phrenic nerve, has been much enlarged, and what was doubtful rendered more certain, by the investigations of the author

As many of the explanations of the functions of the nerve might be unintelligible without an accompanying survey of the anatomical facts, we purpose to connect the two shortly, and to consider the nerve at its origin in the neck, in its course in the chest, and at its ending in the diaphragm, in the serous membranes of the thoracic and abdominal cavities, and in some of the viscera.

Cervical part of the Nerve—A reference to the origin of the nerve may be considered unnecessary at this time, and it would not be introduced here if the writer had not been informed, though the statement is scarcely credible, that there are some members of the profession who still entertain the purely theoretical views of Bell concerning the origin and the functions of the large diaphragmatic nerve At its origin the nerve is but an offset of the cervical plexus, like others to the depressor muscles of the hyoid bone, it is connected with both anterior and posterior (motor and sensory) roots of the spinal nerves, and possesses centrifugal fibres of the sympathetic, as do the intercostal trunks for instance Respecting the number of spinal nerves from which it springs, there has always been much difference of opinion, and the decision of this point by the author, through the numerical method, has some interest For the most part the phrenic comes exclusively from the fourth cervical nerve (twelve times out of thirty-two), and in the remaining instances it was also joined to one or more of the other nerves—being sometimes to only one, at others to both of the two contiguous nerves A difference, as might be expected, has been recognised on opposite sides, thus, on the one side the nerve arose from the third, and on the other side from the fourth spinal trunk, so that injuries of the spinal cord between the attachments of the third and fourth nerves may in one instance only impair the respiration, but in another destroy the action of the diaphragm

The connexions between this diaphragmatic and the other surrounding nerves in the neck have been subjects of keen dispute, but our author's inquiries, with the aid of the microscope, have given much more satisfactory results than the unaided eye and the scalpel could furnish The commonly-received communication with the lower or middle cervical ganglion of the sympathetic is allowed, but the assertions of Haller and Wrisberg respecting the union of the nerve with the vagus are denied, and the occasional apparent junction of the phrenic with the hypoglossal nerve is explained in the following way The intercommunicating offset between the descendens noni and the phrenic was seen only three times out of thirty-two observations, and on unravelling the connecting thread and examining its fibres with a lens, it consisted entirely of nerve-fibres derived from a higher spinal nerve, and not from the hypoglossal or ninth cranial nerve As referring to the loop between the descendens noni and the spinal nerves, it may be stated that sometimes the loop or "ansa" between them is formed solely by the spinal nerves, so that what seems to be a branch descending from the arch of the hypoglossal is only an ascending offset from the connecting branches of the cervical plexus this same arrangement exists in some animals

By means of the close connexion between the diaphragmatic nerve and the other offsets of the cervical plexus, the pain in the shoulder, which is occasionally indicative of inflammation of the liver or its serous covering, receives its explanation, for as the phrenic, and a cutaneous branch of the shoulder, are both derived from the fourth cervical spinal nerve, it is to be supposed, according to our present mode of explaining this and other signs of a like kind in the body, that the stimulus conveyed along the phrenic is transferred in the spinal cord to other fibres of the fourth cervical nerve, and is perceived as if it was felt at the ending of this nerve An example in which the effects are similar is furnished by the trunks of the fifth nerve, where pain in one tooth may occasion pain in all points of the peripheral ending of the trunks of the trifacial nerve

Occasionally Dr Luschka found arched fibres connecting the diaphragmatic with the cutaneous branch of the fourth cervical nerve to the shoulder, similar to those before pointed out by Arnold between the optics, and the posterior and anterior primary branches of the spinal nerves These fibres were directed centrifugally in the two nerves alluded to, and once he succeeded in tracing them one inch and a half towards the periphery of the body, and convinced himself most positively that they were not connected with the central end of the trunk of the spinal nerve The use of these enigmatical loops he cannot explain, for it is said "Respecting the mode of action of these fibres thus unconnected with the central organ of the nervous system, an idea can scarcely be formed it is just probable that they come into contact with ganglionic cells in their course, and take their origin in these '

The Nerve in the Thorax—There is nothing novel communicated, of its course through the thorax to the diaphragm, though the offsets of the nerve to the serous membranes of the lung and heart, and to the heart itself, will supply us with material for subsequent remarks Attention has, however, been directed by the author to a diseased condition of the trunk of the nerve as this lies against the pericardium, connected with the sequelæ of tuberculous deposit, as it is often found injured, after the disappearance of the disease, by the metamorphosis of a new product deposited around it

> "Since" says the author, "I have given special attention to this matter, I find now and then the most marked disturbance of the phrenic during life, accompanied by peculiar symptoms [what?] I have repeatedly made the observation, that by the softening of tubercle and the destruction of the lung substance united with the pericardial pleura, the phrenic was almost destroyed and interrupted in its continuity at a certain point One instance of this I have preserved, in which the right phrenic was so surrounded in one part by calcareous tuberculous material of the hardness of stone, that it was impossible to detach it With these observations before us, we should not omit to give close attention to the disturbances in the functions of the phrenic, as we may explain by them many obscure symptoms in the train of diseases of the chest'

The visceral and parietal parts of the pleura receive nerves from different sources, and possess them in a very markedly different number In the pulmonic pleura they are very few, and are supplied by the vagus at the root of the lung, these are very difficult to detect at a short distance from their origin, and in no case could division of nerve-tubes be recognised Dr Luschka does not appear to confirm the observation of Kolliker respecting the existence of nerve-cells amongst the nerve-fibres, but he does not fail to dissent from the opinions of Bourgery, whose inaccuracy he designates as "den colossalen Irrthum Bourgery's" In the parietal part of the pleura the nerves are more numerous, and are derived from the spinal and the sympathetic thus, in front, there are offsets from the phrenic opposite the third rib, and behind, from the knotted cord of the sympathetic, above, offsets are supplied by the last cervical ganglion of the sympathetic, and below, from the ramifications of the phrenic nerve at the diaphragm The free supply of nerves to the pleura from the phrenic—a sensory nerve in direct communication with the spinal cord and the brain—will, it is thought, explain anatomically why pain in the parietal pleura is so acute, and so aggravated by the movements of the walls of the thorax, and further, the author has no doubt that many of the shoulder pains accompanying pleuritis, and commonly called rheumatic, depend upon disease of the pleura involving the terminal parts of the phrenic A practical application, of more moment as a remedial means, is attempted to be made, from the consideration of the anatomical facts, in the following passage

> "The place of origin of the nerve points out the spot at which derivative or remedial means may be applied for the relief of acute pain There is no doubt that blisters, and narcotic remedies applied to vesicated surfaces, will act most efficaciously above the clavicle, at the spot where direct branches of that nerve are found, whose ramifications are affected at the peripheral termination"

Like the pleura, the pericardium is supplied with nerves from the same three trunks Spinal nerves come to it directly from the phrenic on each side opposite the third rib, and indirectly through the ramifications of the same nerve at the diaphragm Sympathetic offsets are continued to it from the lower cervical ganglion and the loop around the subclavian artery, as well as from the solar plexus through the midriff There is a branch from the vagus only on the right side this arises near the top of the chest, and supplies the pericardium and the upper vena cava In pain in the arm and shoulder in pericarditis, the same reasoning is to be employed in its explanation as is used to account for that in the same parts in pleuritis—viz, the stimulus is conveyed through the medium of the phrenic, upwards to the spinal cord, where it is supposed to be transferred to the sensitive roots of one or more other spinal nerves that are distributed to the spot where pain is said to be felt

The right auricle of the heart and the two systemic veins entering it are supplied by the same three nerves in the thorax The upper vena cava is provided with the branch from the vagus to the pericardium, whilst the lower cava and the contiguous part of the right auricle have separate fine nerves from the right phrenic and mixed nerves, phrenic and sympathetic, from the diaphragmatic plexus And in support of the anatomical facts here announced, the author has made some interesting experiments

> "In dogs and rabbits," he says, "mechanical and chemical stimuli applied to the trunk of the phrenic, isolated on a glass plate, produced very evident contractions in the right auricle of the heart, previously at rest, which were isochronic with the movements of the diaphragm, and not conjoined with contraction of the other compartments of the heart I saw this most satisfactorily in a rabbit that died suddenly through blowing air into the internal jugular vein, and whose heart, distended like a bladder by the air, did not show any trace of movement In this case, chemical stimuli, and pinching the right phrenic with the forceps, caused at once very evident contraction"

The Nerve at its termination—Near the diaphragm, the special nerve of this muscle divides into branches, and many of its ramifications, piercing the muscular fibres, end by joining the sympathetic on the under surface On the right side, a ganglion marks the plexiform arrangement, whilst on the left side it is absent, as we already know from the labours of our countryman, Mr Swan

"A very free intercommunication exists between the phrenic and the sympathetic, and offsets are given to the muscular fibres, to the peritoneal and pleural membranes, to the inferior cava and the right auricle, and to the liver and the supra-renal bodies The points that our space will permit us to notice are only the supply of nerves to the diaphragm, the distribution of the phrenic to the peritoneum, and the action of the phrenic and spinal nerves on the alimentary canal"

Besides the branches from a special nerve, the midriff receives twigs from the six lower intercostal trunks, and from the sympathetic In the fleshy part of the muscle, the nerves have an arched arrangement, and some observations respecting their termination favour the opinion that some end without loops Since Dr Luschka saw many times nerve-tubes lose themselves between bundles of muscular fibres, and though he could not ascertain their exact disposition, he is convinced they did not form loops In the tendinous structure, the ordinary spinal nerve-fibres were perceived, but their mode of ending, in consequence of the fewness in their number and the difficulty of the observation, was not satisfactorily ascertained In the rabbit, where the part is more favourable for microscopic examination, both nerve-fibres—like spinal, and fine granular with the vessels—were easily detected, and were seen to bifurcate and lose themselves in the web, but the manner how, as in man, it was not possible to detect Each phrenic nerve is not confined exclusively to its own side, but sends offsets across the middle line to the opposite side In consequence of this interchange of nerve-fibres, it is conceived that inflammation of the liver, or of the serous membrane of only one half of the diaphragm, may give rise to pain on both sides, near the origin of the phrenic nerve

The chief interest attaching to the supply of nerves to the peritoneum, is the tracing branches directly from a spinal nerve into it these come from the phrenic directly through the fore part of the diaphragm, or from the conjoined phrenic and sympathetic on the under surface of the muscle Some of the immediate offsets of the phrenic are directed along the abdominal wall in a direction towards the navel, and through them, it is suggested, the pain that begins about the umbilicus in inflammation of the peritoneal covering of the abdominal wall will be felt, whilst a greater degree of pain in this spot than in any other will be perceived In a similar way, these offsets are said to originate as a reflex act, the violent paroxysmic vomiting accompanying inflammation of the peritoneum, since the irritation applied to their ending acts through the spinal cord on certain nerves of muscles that are used in the expulsion of the ingesta from the alimentary tube

The author has made some experiments on animals, to ascertain the action of the phrenic on the intestinal canal, and has put forward some views respecting the action of the spinal nerves on that part These views may be interesting in the province of practical medicine, and are contained in the following extract

"It is scarcely to be doubted that the branches connecting the sympathetic with the cerebro spinal nerves conduct the influence emanating in the central parts (brain and cord) to the different organs through the medium of the branches of the sympathetic And it is a dietetic measure, well founded in experience, to bring the abdomen into activity by violent running or other bodily exercise in a congested state of the vessels of the abdomen, in sluggishness of the alimentary canal, or in interruption of the secretory action of the liver Thus, where no structural change is present, but only an insufficient stimulus gives origin to those affections, it appears that an increased activity of the spinal cord is not confined to the voluntary muscles alone, but is continued to all nerves that are in union with it. And as all the spinal nerves communicate with the abdominal ganglia in different ways, and especially through the phrenic (?), so the stimulating influence of the voluntary power, originated in the cerebro spinal system, cannot fail to reach the abdominal organs That such an excitation is produced on the intestinal canal through the intervention of the sympathetic, is rendered more than probable by my experiments In rabbits, the movements of the small intestine were rendered very evident, at different spots, by mechanical and chemical stimulation of the phrenic nerve, even after all motion had previously ceased From the difficulty of judging of the cause that originates the intestinal movement in recently killed animals, I will at present only note these observations, and recommend them to the regard of others ”

With this extract the notice of the work may be closed In this monograph, professing to treat only of the phrenic nerve, there is much that is interesting to the anatomist such as the structure of the vascular secretory fringes in the serous membranes in the chest and belly, the structure of the areolar tissue of the omentum, and the tendinous part of the midriff, with the different kinds of nerves that are found in the peritoneum, &c &c A careful perusal of the original treatise will afford much information in return for the time employed on it

G Viner Ellis

REVIEW V

1 *Die Pathologischen Pigmente* Von RUDOLPH VIRCHOW (VIRCHOW und REINHARDT's 'Archiv für pathologische Anatomie und Physiologie' Vol I p 379, and 407 1847)

The Pathological Pigments By RUDOLPH VIRCHOW

2 *Hæmatoidin und Bilifulvin* Von R VIRCHOW ('Verhandlungen der Physikalisch-Medicinischen Gesellschaft in Wurzburg,' p 305 1850)

Hæmatoidin and Bilifulvin By R VIRCHOW

3 *Beobachtungen uber eine eiweissartige Substanz in Crystallform* Von K E REICHERT, in Dorpat ('MUELLER's Archiv,' p 197 1849)

Observations on an Albuminous Crystalline Substance By K E REICHERT

4 *Ueber das Milzvenenblut* Von OTTO FUNKE ('Zeitschrift für rationelle Medicin, von Dr HENLE und Dr PFEUFER. pp 172—218 1851)

On the Blood of the Splenic Vein By Dr O FUNKE

5 *Neue Beobachtungen uber die Krystalle des Milzvenen und Fischblutes* Von Dr O FUNKE ('Zeitschrift für rationelle Medicin,' von Dr HENLE und Dr PFEUFER Vol II p 198 1852)

Recent Observations on the Crystals of the Blood of the Splenic Vein and of Fishes By Dr O FUNKE

6 *Ueber Blut Krystallisation* Von Dr O FUNKE (Zeitschrift für rationelle Medicin, von Dr HENLE und PFEUFER. Vol II p 288 1852)

On Blood Crystallization By Dr OTTO FUNKE

7 *Ueber Krystallbildung im Blute* Von Dr F KUNDE ('HENLE und PFEUFER's Zeitschrift' Vol II p 271 1852)

On the Formation of Crystals in the Blood By Dr F KUNDE,

8. *The Formation of Crystals in Human Blood.* By E. A. Parkes, M.D. ('Medical Times and Gazette,' 1852.)

9. *Atlas der physiologischen Chemie.* Von Dr. O. Funke.—*Leipzig*, 1853. *Atlas of Physiological Chemistry.* By Dr. O. Funke.

A new element has been recently added to our not inconsiderable stock of bio-chemical puzzles, which promises in many respects to modify the doctrines of the physiologist and the pathologist, and which appears even to affect the entire question of the distinction between the organic and inorganic kingdom. The theory of binary compounds characterizing chemical metamorphoses of the latter, while the former was supposed to evince an exclusive predilection for ternary or analogous combinations, has long since been shown to be untenable. It has still, however, been hitherto held to be the rule, that crystallization was peculiarly a property of inorganic chemistry, and that crystalline forms were only met with in the animal economy, when the material giving rise to them had lost all claim to be considered an integral part of the living tissue. The discovery of crystals which presented all the characters of an albuminous compound, and the subsequent discovery of the crystallizing power of all kinds of red blood, from whatever organ, or from whatever species of animal it may be taken, and the observation of analogous formations under circumstances which leave little doubt of their having been formed during life, renders it probable that a re-construction of some of our present doctrines will be necessary, in order to reconcile the facts which we shall lay before our readers, with our previous views of morphology. As yet we are but on the threshold of the enquiry. Only enough has been done to excite a lively interest. The metamorphoses to which we are about to advert, possess considerable attraction on account of the delicate manipulation required to detect or produce them, the elegance of the forms, and the frequently brilliant colouring characterizing them. The very circumstance, however, of the products being almost exclusively microscopic, has hitherto prevented the enquirers who have engaged in the subject from acquiring very positive data relative to their chemical constitution; hence the evidence in this respect is, as we shall see, mainly of a negative kind.* As we have no desire to prejudge the case, and as the circumstances have as yet met with very little attention in this country, we shall be justified in giving a detailed account of the observations hitherto made, and we shall do so, as far as can be done conveniently, in the chronological order of the publications from which our information is derived.

As it will be convenient to use a designation for the forms which are to engage our attention, and Virchow's term, *hæmatoidin*, has become in a measure established, we shall employ it generically for crystals derived from the blood. In doing so, we pledge ourselves to no theory, and any future determination of their exact chemical constitution will find us at liberty to modify our present views, or to classify under different species, varieties which at present it may be most convenient to regard as identical. The prevailing forms are the rhomboidal and the oblong rectangular, while the pink or ruby colour appears to be a uniform characteristic, the intensity of which may amount on the one hand to black, or on the other fade away entirely

---

* Since this review was written it has been stated by a writer in the Monthly Journal that in a private communication, Professor Lehmann has announced that he has succeeded in isolating the crystals, and in proving their albuminoid composition.

1 It appears that Sir Everard Home was the first to make an observation bearing upon this point In a short tract on the formation of tumours, published in 1830, he gives a drawing of a section of an aneurismal tumour, in which some small crystals are observed a microscopic view represents the latter as well-defined rhomboidal or oblong lamellæ, and their darker shading implies a deeper colour than the surrounding tissues. Sir Everard only speaks of them as the crystallized salts of the coagulum, and does not enter into any further particulars concerning them They are, however, manifestly distinct from the well-known lamellæ of cholesterine

2 The next reference we meet with on this point is in the 'Chemische und Mikroskopische Untersuchungen,' by Dr Joh Jos Scherer (Heidelberg, 1843) In his fifty-ninth case, in which, in consequence of a contusion of the thigh, an extravasation of blood and pus had formed, he examined the blood discharged by an incision a few days after the contusion this exhibited nothing remarkable, three days later, a further quantity of extravasated fluid was evacuated, this was red, and after standing a short time, separated into a reddish-white sediment, and a dark red serum Upon the latter small white shining points were observed, proving under the microscope to be oil globules, the majority of these exhibited an arborescent formation in their interior, and occasionally small reddish yellow rhomboidal crystals Several of the arborescent forms resembling confervæ were also contained in the fluid, external to the fat-globules Dr Scherer expresses it as his opinion that the ramified forms were crystals of margarin, and the rhomboidal crystals cholesterine A further evacuation, made three days later, exhibited only pure pus

3 Zwicky,* in the following year, was the first to examine these crystalline forms more minutely He found them in the corpora lutea of cows, rabbits, and pigs, and classed them under two heads, 1, yellow or chesnut-coloured prisms and needles, 2, square or rhomboidal plates of a more or less intense red He observed that chlorinated water destroyed their colour, that the chesnut crystals were soluble in ether, and he concluded that they must be therefore assumed to be a fatty compound The red crystals were produced by the action of various reagents upon the fat, they were not altered by alcohol, ether, liquor potassæ, acetic, muriatic, or nitric acids, while they were rendered blue and then converted by concentrated sulphuric acid into irregular black balls, which subsequently dissolved with disengagement of gas These results appear so striking, and at the same time so positive, that it is much to be regretted that the experiments have not been carefully repeated and thoroughly sifted, with a view to determine their real value We shall see that Virchow entirely denies the conclusions arrived at, that the crystals are of a saline and fatty constitution

---

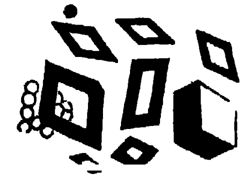
* In his inaugural dissertation De corporum luteorum origine et transformatione; cum tabula. Turici 1844 As we have been unable to obtain this work we give this author's observations on the authority of Virchow in Virchow und Reinhardt's Archiv fur pathologische Anatomie und Physiologie, vol i pp 379, 407 1847

4 Dr Gunsburg* is quoted by Virchow as the next person who observed crystals belonging to the same category, they are described as small cubical octahedra found in a thymus gland affected with fatty degeneration.

5 In the first volume of Rokitansky's Pathological Anatomy (published in 1846, p 170), we find the statement that in the changes that ensue after hæmorrhage has occurred into the tissues, the hæmatin is subjected to a special metamorphosis, it is converted either in the corpuscles, or external to them, into a brown, rusty, yellow, or black pigment, this is seen in the shape of round corpuscles or granular molecules, either free or enclosed in cells, or attached to small prismatic crystals of triple phosphates," and at a subsequent part of the same volume (p 302), the author attributes the varieties of colour, theoretically, in the different pigments, to combinations formed between hæmatin and acids like the carbonic or hydrochloric, giving rise to carbonates, muriates, &c

6 We now arrive at the first comprehensive treatise† on the subject, based upon numerous researches and experiments, and attempting to harmonize the phenomena of crystallization with the known changes in the pigments of the animal economy In the paper alluded to, Rudolph Virchow assumes, with Breschet, Rokitansky, and others, that the pathological pigments are derived from the colouring-matter of the blood He expresses his views thus the diffused colouring-matter passes into a granular form, the hæmatin-containing cells are metamorphosed into pigment-cells Their colour is at first always reddish, subsequently assuming tints which appear to be uniformly the same, according to the organ affected, thus in the lungs and intestines the colour is always black, while in the skin it is orange, or reddish-brown, in the brain, purple, in the vessels, a dirty greenish yellow The effect in the intestines is attributed to the presence of hydrosulphuretted gas Virchow proceeds to describe the granular matter as being rarely spherical, but presenting a well-defined outline, the terminal step of the process of pigmentary formation he regards to be the conversion into crystals, which he finds to occur invariably in extravasated blood He states that he first observed these crystals in 1844, but that his attention was not especially directed to them until the publication of Zwicky's memoir, he has since found them in coagula occurring in the Graafian vesicles, the brain, the skin, and obliterated vessels, in hæmorrhagic infarctions of the spleen, and in abscesses of the extremities He describes the crystals as oblique rhombic columns of a lateritious, yellowish red or deep-ruby tint, according to their thickness, varying much in size, transparent, and occurring free or in cells The crystals occasionally presented the size of ordinary triple phosphates in the urine, some, taken from an old apoplectic clot in the brain, measured 0 0042 lines in length, 0 0021 in breadth, and 0 0055 diagonally from one acute angle to another By apposition of their narrow surfaces, they are apt to form

Fig 1



Virchow's hæmatoidin crystals ‡

---

* See Haser's Archiv 1845, p 104 This is another book not to be found in the medical libraries of the metropolis

† Die pathologischen Pigmente von Rudolph Virchow in Virchow und Reinhardt's Archiv fur pathologische Anatomie und Physiologie vol i pp 379 407 1847

‡ Copy of plate in Virchow's Archiv, 1847

strange figures resembling ribands. These crystals are met with at different periods after the occurrence of the extravasation Virchow has found them on the 17th, 25th, 27th, and 29th days, showing that the process is rather a slow one, and one that appears to indicate a withdrawal of the effused blood from the vital power This coincides with what we observe in the production of crystalline forms in blood removed from the body, to which we shall advert subsequently A most important question, which Virchow determines in the affirmative, is the isolation of the pigmentary matter within the blood-corpuscles Every microscopist knows that pigmentary matter occurs in two main forms, either as free amorphous granules, or enclosed in cells that vary in size and shape, but are considerably larger than the blood corpuscle The most manifest instance is the black matter of the pulmonary tissue, which may be seen as opaque, black, isolated granules, or invested by an oval or circular transparent envelope Again, the coloured compound corpuscles that we find in inflamed tissues or in inflammatory exudations, may equally present the appearance of a mere accidental agglomeration, or the glomeruli may exhibit a sharp outline, indicating the presence of an albuminous sheath The colouring matter of malignant growths exhibits a similar tendency to either mode of deposit The theoretical view of these pigments being derived from the blood, has now been confirmed by the direct observation of Virchow, who concludes his first paper with the following remarks —"The pathological pigment originating in hæmatin may be diffuse, granular, or crystalline, it may assume these forms within and external to the bloodvessels, within and external to cells, it may be yellow, red, or black, or exhibit any of the intervening shades, the hæmatin may have previously quitted the blood-corpuscles, and diffused in other parts, in order subsequently to become aggregated into granules or crystals, the blood-corpuscles may, however, unite directly, combine their hæmatin, and at once form granules or crystals "

In the second paper, Virchow enters more fully into the consideration of the effect produced by reagents upon the pigment-crystals Their analysis is a matter of great difficulty, as no one has hitherto succeeded in entirely isolating them, and in obtaining a quantity sufficient for the determination of their chemical constitution. When we attempt to form crystals from the blood, the material causes the two glass plates to adhere so firmly, that even with the device of previously introducing a hair, it is almost impossible to cause any liquid to pass in, the operculum, with the greatest care, will often break rather than yield to the finest wedge The most satisfactory mode that we have ourselves adopted, is to employ an animalcula case, which consists in an object-glass, fitted in a brass ring, and covered with an operculum, similarly adjusted, by which means it is possible, not only in the first instance to adjust the exact mode of compression, but also to remove one from the other after the crystals have formed Virchow describes liquor potassæ as the most effective reagent he states the colour of the crystals under its influence to become more intense, after which the colouring matter is diffused, and lastly, dissolved The concentrated mineral acids he found gradually to round-off the crystals and destroy them, the colour at first being rendered rather darker, the colouring matter could not subsequently be recovered, and nothing but a light, granular, membranous stroma remained Sulphuric acid gave rise to a most beautiful play of colours, producing successively a brownish or purplish-red, green, blue, violet, red, and yellow Acetic acid does not appear to have exerted any material influence upon the crystals, though it is stated to have occasionally had some effect upon the diffused yellow pigments, and in rare cases also upon the granular yellow pigment Alcohol and ether were found entirely ineffectual From these results he argues that the crystals cannot be of a fatty nature, as maintained by Scherer, and Zwicky, and Henle,* and we gather it to be his opinion, though not formally expressed, that the hæmatoid crystals are a combination of the colouring matter of the blood or hæmatin with protein-matter

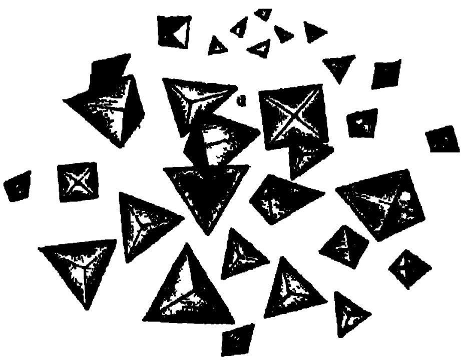
The Transactions of the Medico-Physical Society of Wurzburg contain an article by the same physiologist on hæmatoidin and bilifulvin, which, though not strictly in the chronological order which we are pursuing, may most suitably be adverted to as a sequel to the memoir just considered The author here again enters into the question of the ultimate constituents of the crystals, and their relation to the colouring matters of the blood He commences by observing that he has frequently found the hæmatoidin crystals in connexion with the fat-cells of subcutaneous fat, or attached to free oil-globules, as, for instance, the exudation of a case of hæmorrhagic pericarditis, and in an echinococcus cyst in the liver, in each of these cases bright red spots were seen studding drops of yellowish fat, which, on microscopic examination, proved to be hæmatoidin crystals From other observations, he is led to conclude, further, that fat not only exerts an attraction upon hæmatoidin, but also appears to promote the metamorphosis into hæmatoidin, thus in a case of amputation of the arm, four days after the operation, hæmatoid crystals were observed in the adipose cellular-tissue of the flap The crystallizing process may take place under three different circumstances first, in cells, secondly, in amorphous protein substances (in both cases from diffused hæmatin), thirdly, from heaps of blood-corpuscles, which coalesce with diffusion of hæmatin, and undergo a common metamorphosis Having found the blood-corpuscles to contain, in addition to hæmatin, a protein-substance resembling fibrine (which he terms globulin), and a matter analogous to alkaline solutions of albumen, he infers, that in the blood-crystals, protein invariably co-exists with the hæmatin At the same time he observes, that as all protein substances occurring in the human body contain a small amount of fat, this adhering fat enters into the metamorphosis, and may be present in the colourless nubecula, which remains after the extraction of the hæmatoidin He concludes the subject by maintaining that the crystallizing power is inherent in the protein, and that the colouring matter in the crystals bears no other relation to them than the metallic colours impregnating the crystals of quartz

7 In this view Virchow is supported by the investigations of K E Reichert, in Dorpat, who, apparently without any knowledge of Virchow's first paper, or the other observations already alluded to, in 1849 published the paper which stands third in our list at the head of this article In examining the membranes of the almost mature fœtuses of a guinea-pig, six hours after death, he found a substance upon the placentæ of each of

---

* Handbuch der rationellen Pathologie, vol ii p 738

Fig 2*



Reichert's albuminous crystals

the four animals contained in the uterus, which at first sight resembled dried blood He was much surprised to discover that this substance was composed of tetrahedral crystals, surrounded by more or less mucus or epithelium Though varying much in size, their goniometric relations were identically the same They appeared to be perfectly homogeneous, and when cut through with a cataract-needle they suffered no alteration A further minute examination led him to conclude that these crystals were of an albuminous or protein compound, when exposed to heat on an object-glass, they at first contracted, then carbonized, and were then volatilized, no residue remained after this process, which effervesced with acids They were heated to redness in a vacuum with potassium, and a considerable amount of nitrogen was evolved No change was produced by boiling them with alcohol, æther, sulphuret of carbon, fatty or ethereal oils No acids or bases destroyed them after a brief application, but they were dissolved by the continued influence of concentrated nitric acid, and by boiling in a concentrated solution of potassa When introduced into a strong glass tube, hermetically closed at both ends, and exposed to a temperature of 140° to 160° Cent (252° to 288° Fahr), they were entirely fused The inclination of the planes to one another was 70° 3′ 43″, the height of the central axis from $\frac{1}{17}$ to $\frac{1}{600}$ of a Paris line, though of an intense blood-red colour, they were perfectly transparent The smaller ones exhibited a slightly yellowish tinge The colouring matter he believes not to constitute an essential feature, but to be derived from a foreign pigment A parallel striation was often perceptible on the surface, they had a semi-solid consistency, and were elastic, for when compression was employed they could be reduced to a lamina, and on removing the compression they resumed their previous shape They sank in water, as well as in the acids or alkalies with which they were brought into contact The following is a brief summary of Reichert's experiments with various reagents

Concentrated acetic acid caused the crystals to expand without in any way altering their character, they merely became lighter in colour

Hydrochloric acid the crystals expanded, and assumed a slightly yellow or brownish hue, otherwise unaltered

Sulphuric and phosphoric acids acted like the last

Nitric acid caused an enlargement of the crystals, and the evolution of some air-bubbles

Solution of iodine produced no expansion, but an opacity and a dark-brown colour

Liquor potassæ and liquor ammoniæ expanded and rendered them yellowish brown

---

* Copy of plate in Muller's Archiv, 1849

From these results, Reichert argues that the crystals enter into combination with acids and alkalies, which uniformly enlarge them and change their colour, and that nitric acid converts them into xanthoproteic acid. The application of nitrate of silver, muriate of iron, and prussiate of potash, produced no marked alteration; but when the crystals had expanded under the influence of the acids, they instantly returned to their previous size when water was added. Reichert has instituted a series of experiments upon xanthoproteic crystals, which lead him to conclude that they suffer exactly the same changes under the influence of acids and alkalies as the crystals under consideration; and as it appears that every albuminous substance is converted into xanthoproteic acid by the action of concentrated nitric acid, the inference as to the albuminous character of the former certainly appears justifiable. Gluge* dissents from this doctrine, and assumes that the hæmatoidin exhibits a combination of colouring matter with carbonate of lime; this, however, is based upon only one observation of the evolution of gas-bubbles from the action of concentrated nitric acid, which in itself rather tends to fortify Reichert's view, the more so as we shall find that there is reason to suppose that the mere form of the crystals is, so to speak, accidental, and that while the blood of the same individual may be induced to assume various shapes, certain animals exhibit a tendency to produce peculiar and characteristic forms. It is not impossible that eventually the crystallization of the blood may become a means of predicating its origin, and that a practical application of the fact may be made in medico-legal inquiries, where a doubt exists as to the nature of a sanguineous fluid.

8. We have as yet only traced the formation of crystals from the animal fluids or the colouring matter of the blood *after* its elimination from the blood-corpuscles. Kolliker† is the first who observed them *in* the corpuscles. In the blood of a dog's liver he found a great number of blood-globules, containing from one to five rodlets of a dark yellow colour, which remained unchanged in water, but seemed to disappear under the influence of acetic acid. He next observed the same appearances in the splenic vein of a dog, and of the fresh-water perch, and in the splenic pulp of the latter; the crystalline rods varying from $\frac{4}{1000}$th to $\frac{6}{1000}$th of a line, occasionally of the entire length of the diameter of the blood-corpuscle, at other times less. On the application of water, a membrane was upraised from these small rods, and a nucleus came into view. An accurate examination clearly proved that they lay in decoloured blood globules; and in unchanged corpuscles the gradual formation of one or two of the crystals could be followed. An enormous quantity of really free crystals were found in the spleen of the barbus fluviatilis, sparingly in the kidneys, liver, and the blood of the heart. They were of a nail or spindle shaped form, and of a violet or reddish colour, and dissolved freely on the addition of acetic acid, leaving some colour behind. Kolliker views these crystals as allied to the hæmatin of the blood, and suggests that they may be identical with Virchow's hæmatoidin: he considers their occurrence in the spleen to be of physiological interest. Kolliker, who regards the function of this organ as consisting mainly in a destructive metamorphosis of the blood-corpuscles, would

---

* Atlas der pathologischen Anatomie. 1850. Art. Histologie. Plate I. No. 47, and p. 24.

† The Cyclopædia of Anatomy and Physiology, by R. B. Todd, M.D., Art. Spleen, by Professor Kolliker, 1849.

probably give an explanation to account for the presence of the crystals in unison with this retrograde process

9 The converse theory, that the spleen is rather destined to form new blood globules, is maintained by other physiologists, among whom we espe-

Fig 3 *

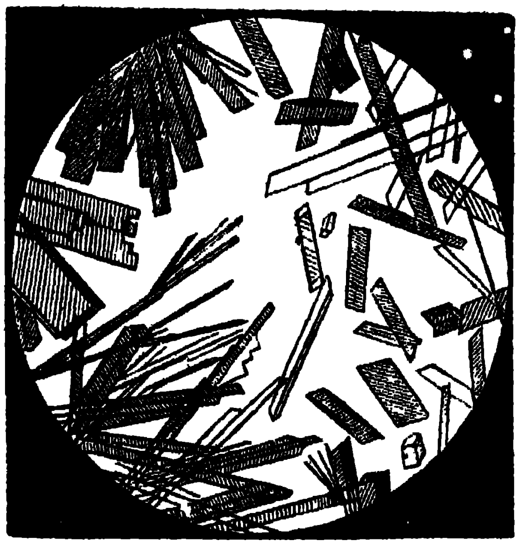
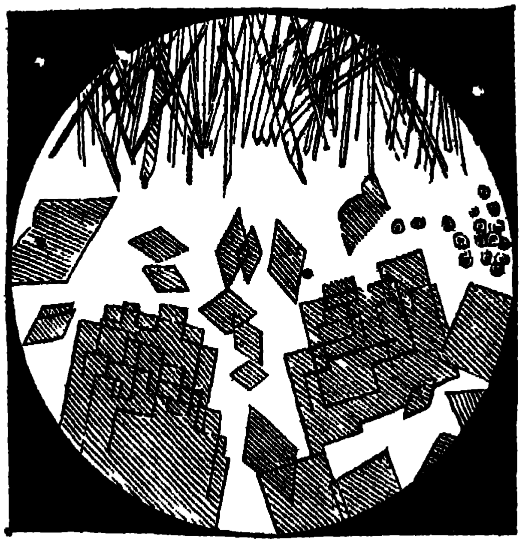


Fig 4 *



cially mention Dr Otto Funke, as his researches into the functions of the spleen have led to his discoveries regarding the crystallizing power of the blood in various animals He justly argued, that to determine the functions of the organ in its influence upon the vascular current, a result would be most satisfactorily attained by comparing the ingoing with the outgoing blood, the arterial with the venous His first series of experiments were performed on the blood from the splenic vein of the horse, in pursuing which he met with a phenomenon which he describes in the following words

> "If we add water to a drop of blood spread out upon the object glass, when it has just begun to dry up in consequence of spontaneous evaporation, and observe the edges of the heaps of blood corpuscles, they are seen to undergo a sudden change A few blood corpuscles disappear, others receive dark thick edges, become angular and elongated, and are extended into small well defined rodlets In this manner an enormous quantity of crystal embryos are formed, which are too small to enable us to determine their shape, they rapidly extend more and more lengthways, while the transverse diameter remains the same, or increases but slightly, the entire field of vision being gradually covered with a dense network of acicular crystals crossing one another in every direction This process takes place with such extreme rapidity, that it is difficult to observe the first origin and the gradual development of the crystals, for this reason, it was only after numerous observations that I convinced myself that the crystals are really formed from the blood corpuscles "

The crystals themselves presented two forms, the one exhibiting an acicular prismatic shape, the other occurring as rhombic plates The majority of the former disappeared after a few hours, the fluid drying up contracted into a few red plates and bands, in this concentrated residue, pale cholesterin-like rhombic laminæ were developed, which varied much in their

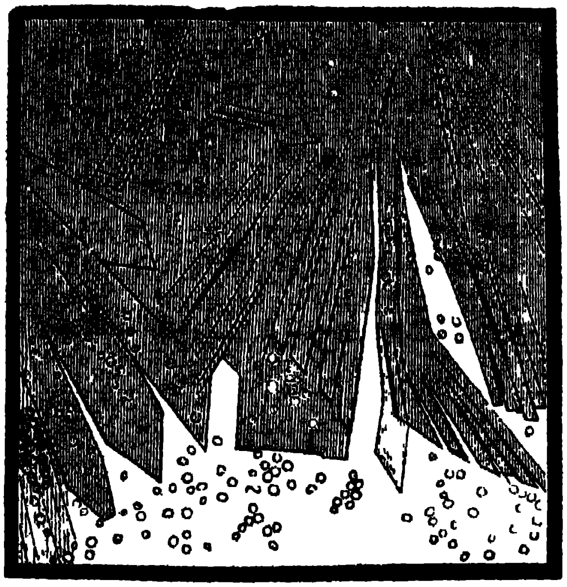
---

* Figs 3 & 4 are copies of plates in Funke's Atlas.

dimensions The acute and obtuse angles measured (by Schmidt's microscopic goniometer) respectively about 60° and 120°

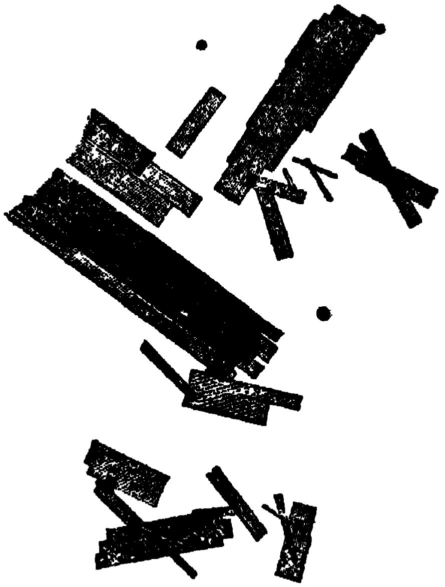
The blood-corpuscles never entirely disappear from the field after the formation of the crystals—an observation which we have also made in our experiments upon the crystallizing power of human blood, they remain upon and about the crystals, and seem to indicate a varying power of resistance to solvents, while their occasionally darker but more frequently lighter hue suggests a different chemical constitution from those which have undergone the crystalline metamorphosis. In the adjoining drawing (fig 5) of crystals, which we succeeded in forming from blood squeezed out of an apoplectic lung of a female, this is attempted to be shown The whole field presented a pinky hue, in which the blood-corpuscles were seen, the crystals were several shades darker, but of the same tint, and upon them were also corpuscles slightly varying in size and depth of hue To exhibit the fact that the crystalline forms vary much according to accidental circumstances, we also annex two other drawings of crystals obtained from human blood. Those in fig 6 were formed by treatment with water, within an hour after putting-up the preparation The blood was a part of the coagulum taken from the heart of a man. At the time this second specimen was prepared, the blood-corpuscles had ceased to be visible The third illustration (fig 7) we subjoin in order to show that the ordinary laminated appearance of the crystals formed in the human blood is a fortuitous circumstance, probably depending upon the approximation of the glass plates between which they have formed The three crystals to the

Fig 5



Hæmatoid crystals formed in blood of a deeply-congested lung on addition of a minute quantity of water and allowing it to evaporate for ten hours under an operculum

Fig 6 *



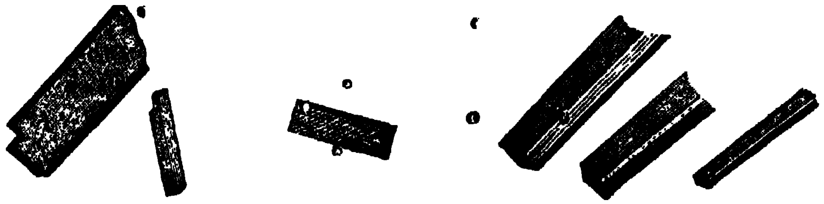
Crystals formed from coagulum in a man's heart one hour after putting up the preparation No blood corpuscles remained but some granular colouring matter was seen in the fluid surrounding the crystals These were transparent and of a crimson hue Magnified 400 diameters

---

* Figs 5 & 6 are copies of drawings by Dr Sieveking.

left in the drawing were formed in a drop of blood squeezed from the apoplectic lung, and though isolated, they presented a deep ruby colour The three crystals to the right of the drawing appear to be hexagonal columns,

Fig 7*



Large ruby crystals formed in human blood

or they may be four-sided prisms, but they manifestly possessed considerable thickness The latter were formed from the same heart-blood used in producing the laminar crystals (fig 6), the animalcula-case was employed in this case, on removing the operculum the crystals were persistent, but instantly disappeared on the addition of acetic acid, and without that brilliant play of colours which liquor potassæ exhibited in one instance in which we were satisfied of its actual application to the crystals In this case the crystals were immediately destroyed, and the most brilliant variation of yellow, green, pink, crimson, and scarlet (no blue), ensued It may be well to add, that the drawings, figs 5 and 6, were taken with a power of about 400 diameters, while for fig 7, one of only about 250 diameters was employed In Funke's experiments, the serum of the splenic blood could not be made to crystallize by any re agents, the same applies to blood taken from other parts, though it appears that Lehmann has succeeded in obtaining a network of acicular crystals from the serum of the portal blood of the horse on the addition of water† In a report of M Kunde's observations, to which we shall shortly advert, in the 'Gazette Medicale' for May, 1852, it is also stated that Messrs Robin and Verdeil produced tetrahedral colourless crystals, like those described by him, from the serum of the blood alone, but no particulars are given

Funke has extended his experiments to the blood of other animals, he has found the same tendency to exist in the splenic-vein blood of the dog, when perfectly recent, showing, as he conceives, that it is not the result of post-mortem changes The blood of fishes is eminently crystallizable, but he failed to obtain crystals from the blood of the splenic vein of the bullock According to his observations, the crystals dissolve easily in all reagents, he met with none in which they did not dissolve, and has failed in all his attempts to concentrate the crystallizable substance We are obliged to omit all reference to other matters treated of in Dr Funke's elaborate papers, though we do not undervalue the importance of his investigations, in their bearing upon the physiology of the spleen We therefore turn to his second paper, in which he more particularly examines the crystallizing power of the blood of some fresh-water fish By treating the recent blood of Leuciscus dobula, Cyprinus erythropthalmus, and Abramis blicca, by partial evaporation, and the subsequent addition, under the operculum, of a small portion of water, he has found that all the blood-corpuscles were converted into crystals, these being generally half or twice as long as the former, their ends rectangular, and their sides rectilinear or slightly curved This phenomenon he explains by assuming that the contents of the blood-corpuscles crystallize without a rupture of the envelope, and that the resolution of the crystal enables the corpuscle to return to its normal shape On the further addition of the smallest quantity of water, the crystals are re-converted into blood-disks Some variations in the mode of crystallization are produced by varying the amount of water added, if it be equal to about half the quantity of the blood, the corpuscles resist its action for some time, after a few hours they appear distended and pale, and finally they vanish At a certain point of dilution, the preparation after a few hours exhibits a close resemblance to the frozen vapour on a window, the entire field is covered with a layer of acicular crystals arranged in the form of plumes Very regular crystallization ensues if equal parts of water be added after partial evaporation, large isolated, well-defined, quadrilateral prisms varying much in size, result They may attain a length of two or three lines, and then become visible to the naked eye Some are as thick as they are long, others so thin and pale, that the vertical lateral planes are not to be recognised If the blood-corpuscles were closely aggregated, the resulting network of crystals was so dense that it resembled a confused mass of needles The colour was vermilion Funke failed to produce the large laminated crystals in fishes which were met with in the blood of horses It does not appear that he has made many experiments upon the crystallizing power of human blood, he states that he obtained crystals in two instances, in which he tested some blood from the splenic vein, in one there was leucocythemia, in the other the spleen was healthy The crystals that resulted presented a double character, there were large numbers of very long, irregular, narrow rods, arranged in pencils, or like antlers, in a drop allowed to crystallize under an operculum, without the addition of water, when treated with water, the blood crystallized slowly, and in addition to the rod shaped forms, two varieties of rhombic plates appeared the one small, very dark coloured, and isolated the other in composite layers resembling cholesterine, red, and almost rectangular Both varieties are considered as belonging to the same crystallometric system, of which the rhombic form would represent the type, and to which all transition forms may be referred

---

* Copy of drawing by Dr Sieveking

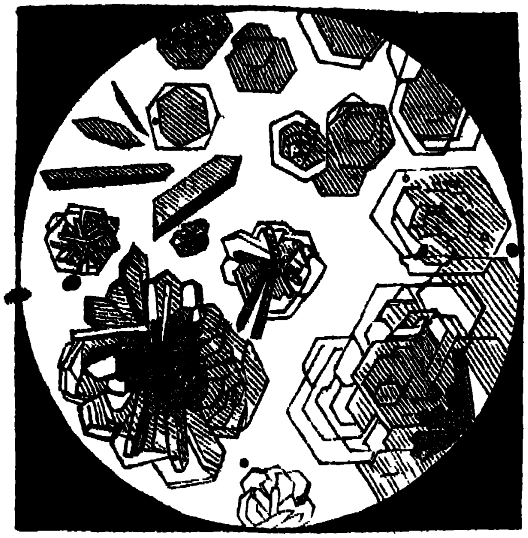
† A paper by Professor Lehmann to which we have not had access is quoted by Dr Funke as containing some experiments on the crystallization of the blood in Berichte uber die Verhandlungen der Koniglich Sachsischen gesellschaft der Wissenschaften der Leipzig, 1852

Funke describes these crystals as extremely destructible, and so undoubtedly they are, but scarcely to the extent he states, for he has found that the mere raising of the operculum instantly converts them into a formless mass, by employing an animalcula-cage, as above mentioned, we have succeeded in maintaining the crystals after removing the operculum, and we are inclined to think that their destruction in ordinary cases is the result rather of the adhesion between the two glasses than from any destructive influence of atmospheric contact Funke supposes the crystallization to depend upon the crystallizable material being dissolved by the water within the envelopes, and being set free by the dehiscence of the latter, to assume its characteristic forms as soon as the fluid attains that degree of concentration in which the crystallizing material begins to be insoluble. If the quantity of water only suffice to distend, but not to burst the envelope, the crystal forms within the latter, and is coated by it, and on the further addition of water the crystals are re-dissolved, and the corpuscles restored to their original form His view as to their constitution does not essentially differ from that of Reichert, in regard to the crystals discovered by him, he considers them to consist of the albuminous contents of the blood corpuscles combined with hæmatin, and he regards them as entirely distinct both from the crystalline forms observed in the blood by Kolliker, and from the hæmatoid crystals of Virchow The only reagents of whose action he obtained satisfactory proof were alcohol, acetic and nitric acids, by each of which they were dissolved.

In the very careful and well-executed drawings contained in the chemico-physiological Atlas, by the same author, the title of which terminates the list of works given at the head of this article, the various crystalline forms derived from the blood of man and animals are represented with the hues they exhibit We can strongly recommend this work to those of our readers interested in the subject, for the drawings are admirably executed, and most faithful representations of nature, while the explanatory letter-press is not overloaded with dry technicalities, the very moderate price further enhances the pleasure of contemplating the evidence of Dr Funke's graphic powers The definite crystallometric character which many would appear to possess, certainly indicates a typical distinction bearing a ratio not yet sufficiently determined to the genus from which it is derived. Many varieties occur in the same blood which may be fairly referrible to the same fundamental type, but we should scarcely be justified in insisting upon an identity of constitution in crystals that respectively exhibit the rhombic, the tetrahedral, or the hexagonal type The blood of the squirrel (fig 8), for instance, forms regular hexagons, which but for their colour closely resemble cystic oxide, the blood of the guinea-pig (fig 9) invariably appears to form tetrahedral pyramids, we have ourselves seen them within two hours after death in a minute renal vessel of this animal, filling up the entire calibre of the channel The case is of some interest, as showing that the contact of the air is not an essential ingredient in the production of these forms In the horse, as in man, the rhombic form may be considered typical, the blood-crystals of the cat are regular quadrilateral or hexagonal columns, while those obtained from fishes (Leuciscus dobula) are more of an acicular character, though also presenting columnar forms

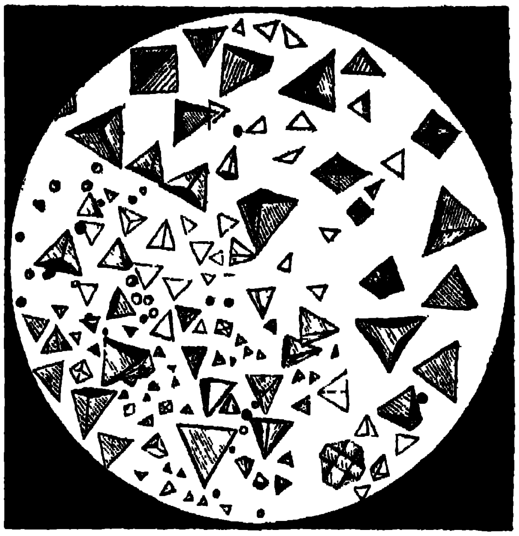
10 Kunde confirms most of the observations made by Funke, though he is inclined to doubt the fact of the crystals being formed directly from the blood-corpuscles, the positive testimony of Funke ought, however, to outweigh a negative opinion, the more so as his confrere has not examined the blood of the same fishes, and as Kölliker has also noted the actual presence of crystals within the blood-disk The animals whose blood Kunde has experimented upon are the ox, the horse, the dog, the guinea-pig, the squirrel, the rat, the mouse, the bat, the rabbit, the pigeon, the turtle, the leech, and the frog In all, excepting in the frog, he succeeded, though with more or less difficulty, in producing crystals, and if we admit the exception as

Fig. 8



Crystals formed from the blood of the jugular vein of the squirrel.

Fig. 9



Crystals obtained from the blood of the guinea-pig.*

proving the rule, we may assume with him that all red-blooded animals exhibit this property. Funke states that he has occasionally obtained crystals by treating a drop of the recent blood from his own finger with water. In this Kunde has failed. In several experiments of the same kind, we must confess that we have also been foiled, while we have met with little difficulty in securing the most beautiful crystalline forms from human blood taken from the lungs or the heart of the dead subject. The effect of reagents upon the crystals, and especially upon those produced from the blood of the guinea-pig, though the latter were identical in form and colour with those spoken of by Reichert, induced Kunde to assume an essentially different constitution between the two. Every chemical applied, dissolved the blood-crystals, and they were soluble in water at a comparatively low temperature (40°—45°C.=62°—71°F.), while it will be remembered that Reichert's crystals exhibited a very indestructible character. Nor has Kunde ever found the crystals pre-formed on the mucous surfaces, but he has observed that if the liquor amnii of a guinea-pig be allowed to evaporate a little, and then covered with an operculum, crystals result that closely resemble hippuric acid; the day after the preparation was put up, regular, colourless, transparent cubes and octahedra were found, adjoining crystalline masses of an irregular outline. The question whether the blood-crystals are identical in different animals, is answered by Kunde, as we should anticipate, even from the manifest difference in the facility with which their blood crystallizes, and the distinct types exhibited, in the negative. Their solubility in water is greater in man than in the guinea-pig and squirrel, and those of the latter are found to be insoluble in perfectly cold water, as well as in alcohol and ether

---

* These appear identical with the albuminous crystals above described as having been discovered and analyzed by Reichert. The crystals form easily and are a pretty microscopic object. The tetrahedral is the prevailing form, and most of the irregular aggregations that we meet with in our experiments are referrible to this type, though we have not seen any transition forms which we could look upon as connecting links between the tetrahedral and the polygonal, of which a specimen is seen at the lower margin of the field in fig. 9. Figs. 8 & 9 are copied from Funke's Atlas.

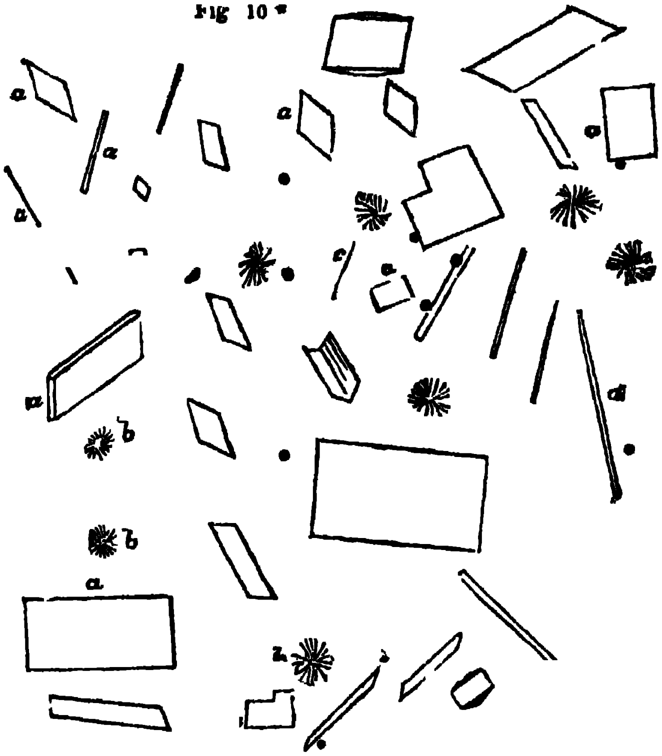
11 The only records of blood crystals given by English observers are contained in a paper by Dr Sanderson, on the metamorphosis of the coloured blood-corpuscles, in the 'Monthly Journal of Medical Science' for 1851, and in an article by Dr Parkes,* on the formation of crystals in the blood, in the 'Medical Times and Gazette' of 1852 The former, in summing up the modifications which the blood-corpuscles undergo in extravasations, remarks, that in rare cases, a crystallizable fat being present, the colouring matter unites with it so as to form crystals He gives three instances in which he discovered rhombohedral crystals of a deep yellowish red colour, attributable to a metamorphosis of the blood, one in a cyst beneath the scalp of a pigeon, another in the corpus luteum of a cow, and the third in the corpus luteum of a human female, calculated to be five or six weeks old The latter is described as containing a bright red centre of tolerably firm consistence, presenting under the microscope a fibrillating structureless material, in which were imbedded large numbers of decoloured blood corpuscles, others more or less converted into grains of pigment, spherical or ovoidal bodies of about $\frac{1}{4000}$ inch in diameter, and of a brick-red colour, and crystals of a rhombohedral form, and very regular, but of small size Intermediate between these, there seemed to be semi-crystalline bodies, as well as irregular masses, all of which consisted of the same brick-red pigmentary material, which was unaffected by acetic acid, diluted mineral acids, or caustic potash

12 Dr Parkes' paper details the appearances observed in blood that has been allowed to undergo partial decomposition In one case some half putrid, half-liquid blood, obtained by venæsection, exhibited, after the addition of water, prismatic crystals of various lengths, cut off sharp at the ends, or tapered, they occasionally appeared to emerge from the sides of a large red mass, which was also penetrated in all directions by them, and which seemed to have been originally formed of an accumulation of red corpuscles, but no perfect red corpuscles could be seen In another case, blood drawn from the arm, and allowed to remain in a stoppered bottle for three months, until it was covered with a thick crust, exhibited in the subjacent oily purple liquid myriads of more or less deep-red crystals of various forms, most of them tables of some thickness, others thinner or heaped together so as to look like cholesterine, others long and rod-like, with straight or acuminated terminations, readily dissolving in water, and destroyed by exposure to air (Fig 10) Mixed up with these were circular agglomerations of acicular crystals, apparently consisting of margaric acid, and insoluble in water, though soluble in ether Again, some defibrinated blood was exposed to the air until it had assumed a syrupy consistence, this showed no crystals until water was added, when, after two or three hours, numerous needle-shaped and rod-like crystals were produced, which were entirely soluble in acetic acid The same blood being allowed to dry, and water being then added, no crystals were visible after

---

* In Schmidt's Jahrbuch 1852 No 12 p, 383 it is stated that Dr Parkes' paper is an account of Funke's and Kunde's observations This is erroneous as the paper was written before Funke's second paper reached England and appeared before his third paper and the paper of Dr Kunde (in which the crystallizability of all sorts of blood was recognised) were published in Germany

Fig. 10.*



a, a Soluble in water. *b b* Insoluble.

two days; but after a lapse of five days a few acicular crystals and numerous margaric-acid crystals became apparent. In a specimen of blood taken from the jugular vein of a man who died of Bright's disease, and allowed to stand for a fortnight, until it had become ammoniacal, the microscope detected triple phosphates, small rod-like crystals that were insoluble in acetic acid, and margaric acid crystals. In other experiments Dr. Parkes has observed large pale crystals of an irregularly prismatic form, resembling some kinds of phosphates. He points out that if we exclude the known forms of margaric acid and the earthy phosphates, three varieties of crystals remain, which are distinguishable:

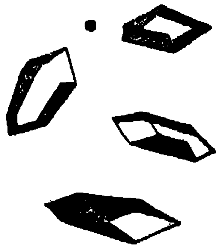
1. By perfect solubility in water.
2. By insolubility in water, by the addition of which they are produced, and by solubility in acetic acid.
3. By insolubility in water and acetic acid.

The foregoing analyses rather exhibit the complicated nature of the question we have to deal with, than enable us to arrive at a definite conclusion already; but we would suggest, that until good grounds be shown for an adverse opinion, we should not treat the forms discovered in recent blood as necessarily identical with those found in blood that has manifested symptoms of evident decomposition. We are inclined, from our own observations, to infer that while under certain conditions recent blood exhibits a tendency to form crystals, a period arrives at which this tendency disappears, and is replaced by a second stage of crystallization, in which the ordinary affinities of inorganic nature come into play. In the sequence of time we must establish an earlier stage of crystallization than either of these two—viz., that in which it is manifested in the living body itself. Here the tendency of the blood to give rise to crystalline forms, exclusive of those of the well-known salts, is evidently greater than has been hitherto supposed. In the human body we have repeatedly met with crystals that we were justified in regarding as pre-formed during life. In two cases of enlarged spleen, and one of a melanotic character, that fell under our notice in 1852, we were surprised to find a large number of rhomboidal and prismatic crystals (figs. 11 and 12), chiefly in the vicinity of the capsule,

---

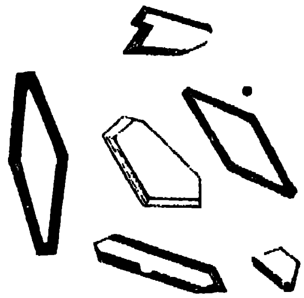
* Copy of plate in Medical Times and Gazette.

Fig. 11.



Crystals found under the sheath of a spleen of normal size in a man æt. 70. Magnified 400 diameters.

Fig. 12.*



Crystals found in a spleen containing melanotic deposit, somewhat enlarged and firmer than in the normal condition. Magnified 400 diameters.

which were imbedded in the soft tissue of the organ, but only exhibited a slightly yellowish appearance, which seemed to be merely a reflex of the adjacent tissues. They did not present the definite cherry or crimson hue displayed by Virchow's crystals, or the blood-crystals that we have succeeded in forming from the blood. They disappeared on the addition of acetic acid, with an evolution of air-bubbles, but were not altered by liquor potassæ. We have since examined numerous spleens, with the view to examining the nature of these crystals more minutely, but have only recently succeeded in again meeting with them, though without being able to isolate them, or define their constitution. Here they were accompanied by numerous opaque, black, rectilinear bodies, differing in size and outline, but generally approaching a quadrilateral form; concentrated sulphuric acid did not change them. The well-defined linear margin clearly distinguished them from the amorphous forms in which black pigment ordinarily appears, either in the lungs, the intestines, the spleen, or other organs. While, however, some of these formations leave a doubt whether they are directly derived from the blood, we meet with other crystals in pathological products resembling blood-crystals, where they are manifestly not immediately deducible from this source. Thus, in a case of recent pleuritic effusion of straw coloured fibrine, investing the lower left lung of a man aged forty, among numerous prismatic crystals, was a crystalline mass of reddish-yellow colour, apparently composed of several layers of rectangular laminæ. Acetic acid readily dissolved the various forms. In the same case a drop of blood from the lung, covered by an operculum, was found after twelve hours to exhibit numerous rectangular and prismatic colourless crystals, which apparently were not touched either by acetic or muriatic acid

---

* Figs. 11 & 12 are from drawings by Dr. Sieveking.

It does not appear that the coloured rhomboidal crystals of Virchow are capable of being artificially formed In our experiments of producing blood-crystals from human blood, we have experienced no difficulty in forming the laminated rectangular and prismatic crystals described and delineated by Funke, and on one occasion, while fan-shaped aggregations of these were still visible in the same field, there were several delicate, well-defined, perfectly colourless rhomboids, which contrasted strongly with the brick-red material surrounding them The quantity of blood employed has a marked influence in determining the arrangement of the crystals, as well as their dimensions In following the directions of Funke, we commonly obtain asteroid groups, or long rows resembling a paling, of well-defined crystals of a laminated character, of considerable length and cut off square or in lance points It is, however, not an essential character that they are merely laminated, for the same blood may yield larger crystals, in which the edges exhibit considerable thickness, or in which the prismatic character is undeniable In a successful case the crystalline forms occupy so large a space, that this circumstance alone would justify the conclusion that they are composed of the bulk of the material spread out on the object-glass The further evidence that has been detailed seems to warrant the inference, that while there are essential differences between many of the forms hitherto described as being derived from the blood, either within or external to the body, they exhibit a tendency of a protein compound to crystallize, not hitherto admitted

E H Sieveking

REVIEW VI

Ueber Geburtshulfe und Gynækologie in Frankreich, Grossbritannien, und Irland grossentheils nach Reiseergebnissen Von Dr F H ARNETH, d Z suppl Primarius am k k Gebarhause, und an der Abtheilung für Frauenkrankheiten, &c &c —*Wien*, 1853 8vo, pp 360

On Midwifery and Gynecology in France, Great Britain, and Ireland, chiefly drawn from Observations made during Travels in those countries By Dr F H ARNETH, at the time principal Assistant Physician to the Imperial and Royal Lying in Institution, and to the department of the Hospital appropriated to the Diseases of Women, Vienna

PERHAPS the most striking characteristic of the present age, as distinguished from all previous times, is the extent to which international communication is now happily being carried, men are, in a sense and to a degree which never before were possible, running to and fro, and knowledge is, as a necessary consequence, being largely increased The earth itself promises ere long to be, in yet another mode, bound into one vast organism, and human thought to be transmitted between the most distant climes, on the chords of a newly developed, as it were, nervous system And there are probably none who must benefit more from the daily increasing facilities for the interchange of ideas and of the knowledge of newly-ascertained facts, than the students of an experimental and inductive science like medicine But not the least interesting or useful circumstance connected with the state of things we have alluded to, is the power we derive from it of " seeing ourselves as others see us — in other words, of learning the impressions made by our views and institutions upon foreigners, and of reading, in the mirror which they hold up to us, the history of our defects and our advantages

For such reasons we hail with pleasure the appearance of a work by an intelligent and impartial observer like Dr Arneth, chiefly devoted to an examination of the medical institutions of these islands For although the title page professes merely to embrace the obstetric branch of medicine, the author does not confine himself to this department, but enters into a review of the systems of education both professional and general, and of the colleges and hospitals of the several countries he has visited, and concludes his work with a comparison of English and German midwifery practice

In the preface we are informed, that after the author had spent three years as assistant physician to the great Lying-in Hospital of Vienna, he undertook a journey through Germany, France, Great Britain, Ireland, and Belgium, in order, by personal observation, to learn the state of midwifery in those countries As time unceasingly advances, and as much of what he has seen and described may in a short space have undergone great alteration, he thinks it right to mention, that his visit to Paris occurred in the months of November and December, 1850, and January, 1851, to London in January and February, to Dublin in March, to Edinburgh in April and May of the latter year

The author first enters upon an examination of the systems of medical instruction prescribed at the three universities of Paris, Montpelier, and Strasburg, and at the secondary schools of France, which he afterwards compares with the plan pursued in Austria. He is most struck with the shortness of the period of clinical medical instruction required in the former empire (one year against two in Austria), and with the entire omission of ophthalmic surgery, which in his country is so much attended to, and has so properly its separate chair and clinique. On the other hand, midwifery, which in Austria is unaccountably struck out of the official plan of study, is obligatory in France, and is taught theoretically during six months, and practically during an entire year. He cannot understand why materia medica and pathological anatomy, which in Austria are placed at the commencement of the practical studies, of which the latter branch of science at least is one of the chief pillars, should in France be deferred to the last half-year.

Hospital visiting is to commence in the second session, in order that the student may become acquainted with the details of so-called "minor surgery." This and other regulations show a desire on the part of the French medical legislators to favour practical studies, and may partially compensate for the short period of proper clinical instruction above alluded to.

The uncommonly great number of examinations and "concours," which continue in an uninterrupted chain during the whole period of study, and are afterwards resumed for those who wish to obtain appointments, is characteristic of France, and quite corresponds to the genius of the people, and may give a certain readiness in disputation, which, while it is in its higher degree neither always necessary, nor a proof of a greater amount of knowledge, may have its advantages, and probably has advantages for so mobile a people as the Gauls, though it seems to be less suited to the German character; at the same time, the author observes that he cannot deny that he has seen similar regulations in operation among the less excitable Britons, and apparently with favourable results.

In the examination for the degree of doctor, the French regulations appear to the author to be in many respects preferable to those of his own country, and chiefly in this, that while in both countries the necessity of making the examination practical, and not merely theoretical, is recognised —in Austria the examiner represents a patient, and submits to be questioned by the candidate, who is required, from the description he obtains in answer to his queries, to state his opinion as to the diagnosis, prognosis, and treatment of the disease under which the examiner supposes himself to labour, and within a quarter of an hour at least to name the disease; while in France, on the contrary, he is brought to the bedside of one or more patients in the hospital, where not only his *knowledge*, but his *power* of using his knowledge, are practically tested; the superiority of this latter mode is obvious.

In France, the "faculty" consists only of the college of professors, with the Dean elected from among themselves at their head; the other doctors form no part of the corporation, and consequently exercise no influence on the renovation of their order. In England, on the other hand, the right to practice is only obtained through admission to a society (College of Physicians, College of Surgeons, or Apothecaries' Company), the members of which are but exceptionally professors Both the Austrian and French regulations have the advantage over the English of uniformity in requirements from candidates for practice, while each of the English corporations has its own regulations, whence arises the great variety to be found in this respect in the British isles

In 1851, 1352 doctors of medicine were practising in Paris, of whom the amazing number of 366 were decorated with the Order of the Legion of Honour

A great advantage enjoyed by France is the simplicity of the division of the profession, there being since 1803 only doctors of medicine and of surgery (the education of whom is quite similar, with the exception of a slight difference at the *last* examination, according as the candidate intends to devote himself to one or the other branch of science), and officers of health, which contrasts favourably with the separation in England into physicians, surgeons, general practitioners, and apothecaries, and in Austria into doctors of medicine, doctors of surgery, and masters and patrons of surgery In France, the members of the more highly educated branches, the doctors, far outnumber the officers of health, while in England the apothecaries, and in Austria the patrons of surgery, whose requirements are least, are unfortunately the most numerous

Having thus given a sketch of the state of medical education in France, the author next proceeds to describe the hospitals of that country, dwelling more particularly on those devoted to midwifery The first he notices is the Strasburg hospital, which has the great and rare advantage of containing a clinique for children The lying-in house of this town is divided into two parts, "la clinique" for physicians, and "le service," in which midwives are instructed, the deliveries amount only to about 120 in the year

Dr Arneth next conducts his readers to Paris, the general hospitals of which city collectively contain 3420 beds, while those for special objects possess 2784 of the latter, the Hôpital des Enfans Malades has 600 beds, La Maternité 514 In the Hôpital des Cliniques there are also wards appropriated to midwifery under C Dubois The direction of the studies both at the "Clinique," where medical students are instructed in practical midwifery, and at the Maternité, where midwives are educated, is placed under the eminent Paul Dubois

In none even of the smaller Parisian institutions did Dr Arneth find the retirement, stillness, and domestic appearance which characterize many of the English hospitals The surprise which the splendour of the wards at first excites in strangers soon gives way to unfavourable impressions created by the over crowding and imperfect ventilation of the apartments, and the want of cleanliness in the inferior attendants

The lying in women in the Maternite are lodged in two great wards, each divided by partitions into thirty-six small apartments, resembling cells, the arrangements for ventilating which are so very defective, that the "puerperal odour" prevails in most disagreeable intensity, and there can be no doubt that this want of sufficient ventilation is a main cause of the great mortality for which these apartments are so notorious A table is given at p 46, showing the comparative average mortality in the three largest lying in hospitals of Europe—viz, the Maternité of Paris, and the hospitals of Vienna and Dublin, for the years 1828—1849, both inclusive in the first-named institution it amounted to 4 18 per cent , in the second, to 5 35, and in the third, to but 1 34 * In a note to p 45 the author points out the curious fact which appears to be established by ample statistical observations, that proportionally more boys are the result of a given number of legitimate than of illegitimate births

Dr Arneth observes, that as the midwives at the Maternité do not apply themselves to anatomical investigations, the prevalence of puerperal fever at that institution proves that the disease is not exclusively attributable to the examination of patients without sufficient attention to washing after dissections, but that its source is principally to be found in the morbific matters impregnating the atmosphere which surrounds the patients

It appears from the table, that the most unfavourable years at each of the three great hospitals by no means coincide—a fact which would show that the opinion of those who hold that a peculiar constitution of the air capable of producing puerperal fever prevails at certain times simultaneously throughout the whole of Europe, is incorrect The same want of agreement has been observed in the two divisions of the Vienna hospital, and also between the "Clinique" and the Maternité, at Paris

The great lying-in hospital of Vienna, which dates from 1784, is situated in the general hospital, and is divided into two parts, one of which is appropriated to the instruction of medical students, the other to the education of midwives The former usually take out their practical midwifery at the end of the fifth year of their medico chirurgical studies Having previously attended a six months' theoretical course on the subject, they spend two months in this practical school, but this is not obligatory on all medical students, nor does practical midwifery form a subject of examination for the degree of doctor of medicine and surgery, which consequently confers no right to practise this branch, in order to obtain the degree of "Magister artis Obstetriciæ," they must, previously to admission to the special examination, attend a second practical course of two months' duration

The author next devotes fifty-two pages to an examination of the differences in practice between the French and German obstetricians, which he prefaces with a description of Paul Dubois, who has for years been at the head of the two greatest midwifery institutions of France, and who appears to reign as autocrat in that branch of science, for in France, observes Dr Arneth, there is no obstetric republic, all standards are lowered before Dubois—a position the more singular, as a similar supremacy had been yielded to his father before him Interesting and useful as an investigation of this important portion of Dr Arneth's volume would be, it includes so great a variety of subjects that we must content ourselves, on the present occasion, with simply enumerating the principal points he passes in review They are—incision of the rigid os uteri, operation for hare-lip, spontaneous and unaccountable, sometimes fatal, hæmorrhages from the nose, mouth,

---

* Through the kindness of the master Dr Shekleton we have obtained from Dr Sinclair senior assistant-physician to the hospital a statement of the mortality at the Dublin Lying in Hospital for the three years 1850—1852 which have elapsed since the period at which Dr Arneth's table terminates During that time the deliveries have been 6012 the deaths 38—i e one in 158 21 or 0 632 per cent Of the 38 deaths however 13 although occurring soon after delivery were the result of non puerperal causes as phthisis, pneumonia, acute bronchitis typhus perforation of stomach, destitution, &c.

and anus of new-born infants, incisions of the perineum in cases of its extreme tension, observations of Dubois and others on congenital syphilis, an examination of Dubois' and Simpson's views on the subject of the attitudes and positions of the fœtus in utero, induction of premature labour, section of the symphysis pubis, the forceps and its application, perforation, the Cæsarean operation, including an examination of the facts brought forward by Dr Hoebecke, of Brussels, who is said to have operated in thirteen cases with preservation of the mother's life, and Jobert's (de Lamballe) operation for urinary fistula

The second division of Dr Arneth's work commences with some general remarks on the hospital system of Great Britain and Ireland

> "It is," he observes, "an ancient fundamental principle of the English government, to interfere as little as possible in the domestic circumstances and affairs of the country This principle is likewise adhered to in the case of hospitals, the erection and maintenance of which are, with few exceptions, left to the people, and to the benevolence of private individuals, consequently, the government exercises no influence over their administration" (p 121)

The advantages of this plan are, according to the author, that the people, who are necessarily best acquainted with their own wants, are entrusted with the management of the institutions intended to relieve them, and that this self-government is much prompter, simpler, and cheaper, than any other It is, moreover, well suited to a wealthy community like that of England, in which gifts have been made of amounts of property seldom attained to in other lands, opening as it does a channel for the exercise of true beneficence, at the same time that it gives the donor a voice in the disposal of his bounty, and through the system of admission into hospital by recommendatory notes, and the consequent direct support by the rich of the poor in the hour of their greatest need, it forms, adds Dr Arneth, "one of the many bonds which, in this remarkable country, unite the needy to the wealthy and the noble"

On the other hand, he admits that the hospitals are thus rendered liable to be affected by changes in the circumstances, the residences, or the goodwill, or by the death, of their benefactors, and to be prevented, for want of a fixed income, from undertaking many expenses The patients are from this cause often limited to a much smaller number than could otherwise be accommodated Other objections mentioned by the author are—possible delay in procuring a recommendatory note, and the embarrassment which a non-professional committee may experience in selecting the persons best suited to fill the medical offices of the institutions "To this," he says, "the English answer—what certainly would speak for the tact of the public and of such committees—that *all* the men who were afterwards distinguished for their science, have been elected as hospital physicians," but in estimating the value of this statement, it must be recollected, that hospitals, if properly used, are the ladders to fame

Coming from the French hospitals, with their showy floors and gaudy display (*Flitter*), the author was much struck with the absence of decoration, and with the uncommon simplicity which prevails in England Low, unornamented beds, without curtains, stand far apart in small number, and offer a striking contrast to those in the enormous and brilliant wards which in Paris and Strasburg are so incredibly filled with patients

The open fireplaces, excellent ventilation, and the chapels attached to the hospitals, next attract the author's attention, the latter are the most highly-ornamented part of the houses, but the tablets which decorate their walls, setting forth the names of the benefactors of the institutions, and the sums they have contributed, have in his eyes a singular and almost profane appearance

The author observes, that the dispensaries usually attached to the hospitals compensate in some measure for the deficiency which exists, at least in London, of places of shelter open for the needy He was particularly struck with the vast numbers which sometimes resort to these dispensaries

Dr Arneth concludes his chapter on the English hospitals by expressing his admiration of the museums of anatomical preparations and plaster casts, often of singular beauty, generally connected with them, and partly formed by the contributions—legacies, as it were—of their former medical officers Thus, that at Guy's Hospital contains preparations by Sir Astley Cooper, and St George's has been enriched by the labours of Sir Benjamin Brodie He points out the great advantages presented by these museums in contrast to many similar collections on the continent, in the liberality with which they are thrown open to students, and the free use accorded to the pupils of numerous copies of perfect catalogues of their contents

The next chapter, on the state of the medical profession in the United Kingdom, opens with an expression of surprise at its division into the three grades of physicians, surgeons, and apothecaries, and at the diversity of the standard of qualification required by the several licensing bodies The author finds fault with the short period of actual study required by the Apothecaries' Company, thinks it strange that they should consider their pupils fit to attend lectures on midwifery and diseases of women and children, at the end of merely a six months' course of chemistry and anatomy and physiology, and that, notwithstanding the very short time allotted for the acquirement of professional knowledge, they should prescribe both a winter and a summer course of morbid anatomy He further reprobates the union in the same individual of the prescribing and sale of medicines, remarks on the professional etiquette which obtains in London of committing internal diseases to the physician, and entrusting the surgeon with only such as are external and in proof of the stringency with which this is observed, quotes the anecdote of a physician who, travelling in a railway carriage, declined to bleed his intimate friend, who was suddenly stricken with apoplexy, but in spite of the danger of delay, sent to a distance for a surgeon, and when asked his reason for so doing, appealed to professional usage Dr Arneth prophesies the gradual destruction of this partition wall, and observes that though but a short time has elapsed since accoucheurs have been admitted to the College of Physicians, he, at the period of his visit, found at the head of both the Irish and Scotch Colleges, men exclusively occupied with obstetric practice

"It is scarcely comprehensible to strangers," observes the author, "how the British, who so warmly insist upon their independence and personal liberty, should so frequently allow themselves to be sold to their physicians" In proof of the existence of such a state of things, he quotes some advertisements from the last page of the 'Lancet, and points to the establishment of a Medical Transfer Office in London, as evidence of the frequency of the sales of "practices" Equally little can he understand the system of partnerships, and that a patient, in answer to his summons, should be liable to see one or other of the partners, as either might chance to be at home It struck him as singular that both customs should prevail more in England than in Scotland, and that in Ireland they should be almost wholly unknown

The author observes that a considerable majority of the English physicians, and also many of the surgeons, have a great advantage over those of his own country, in belonging to wealthy families, and so obtaining a more careful education, and acquiring from their college connexions a better standing in society

The author's remarks on the elementary and university education of future physicians are of general rather than of professional interest, we shall therefore not dwell upon them, but pass to the chapter on the hospitals of London On this subject he has little to add to what he has already said when speaking of the English hospitals in general, but he bears testimony to their great cleanliness and neatness The obstetric institutions of the capital of the world he divides into three classes, first, *Lying in Houses*, of which there are five The British Lying-in Hospital is the oldest, having been founded in 1749, the number of patients is small, and it is little used as a school of midwifery The City of London Lying-in Hospital is the largest and handsomest midwifery hospital in the metropolis, it is roomy and well-ventilated, the author gives a table of the number of deliveries and of deaths which occurred in the house during the years 1827—1850, both inclusive, and from it draws the following conclusions —

1 That the mortality, even in small, admirably situated, and well-kept lying-in hospitals, is always greater than among women delivered at their own houses Thus, while 1 in 70 died in this truly "model hospital," it appears from the official report that but 1 among 171 puerperal women died in the entire of England during the years 1838—1841, and the Royal Maternity Charity states its mortality at but 1 in 222 deliveries It would therefore appear that, on the whole, the poor woman in her hovel, badly or not at all attended to, often in the greatest filth, passes through her confinement more safely than in a healthfully situated house, which to her must seem a palace, surrounded with every care and attention, bedded with the most scrupulous cleanliness the cause of this must lie in the hospitals themselves—must, if the expression may be allowed, be something endemic

2 That the mortality varies extremely in the several years Without wishing to deny the effects of atmospheric influences, the author cannot consider these as sufficient to account for the diversity, for the worst years at the City Hospital do not afford correspondingly unfavourable results from other similar institutions Dr Arneth here again refers to the occasion on which puerperal fever raged so fearfully in the Clinique at Paris, while there was not one sick in the neighbouring Maternité

3 A comparison of the tables furnished by this and the other small lying-in hospitals of London, with that of the great institution in Dublin, shows that large lying-in hospitals do not necessarily, as such, demand more victims than small ones While one mother in 51 died at the

British Lying-in Hospital, 1 in 70 at the City Hospital, and 1 in 59 at Queen Charlotte's, in the large Lying-in Hospital of Dublin there was, under Dr Collins's mastership, but one fatal case among 100 deliveries, and while Drs McClintock and Hardy were assistants under Dr Johnson, the mortality was but one in 102 *

After a short description of the remaining lying-in hospitals of London, viz, Queen Charlotte's, the General Lying-in Hospital, and Queen Adelaide's, the author passes to the second class of the obstetric institutions of the metropolis, those, namely, which provide for the delivery of women at their own homes, the plan which throughout all England appears to be most popular The society which has the most extensive sphere of action in this way, is the Royal Maternity Charity We have already stated the mortality shown by its tables

Dr Arneth's third category contains the workhouses, which afford a refuge for those who are rejected at almost all the special lying-in institutions—namely, the unmarried

The author deplores the great deficiency of opportunity of practical instruction in midwifery consequent on the want of large lying-in hospitals in London—a circumstance which sends most medical students to Dublin or Paris, in which latter city, especially, many English are always to be found in search of that obstetric education which is denied them at home

Dr Arneth next remarks on the very defective instruction of the midwives, whose practical education usually embraces no more than three or four months spent at one of the small hospitals of London, and he contrasts this with the case of the midwives of Paris, most of whom, having been taught with the greatest care during an entire year, in an institution in which three thousand births annually take place, voluntarily remain during a second twelvemonth The author concludes this portion of his volume with an enumeration of the names and writings of some of the most eminent of the accoucheurs of London

The general hospitals of Dublin, to which city the author now transfers his readers, contained at the time of his visit, on the whole, about 1060 patients Among them, Sir Patrick Dun's was distinguished by its neat and pleasing appearance

> "The practical turn of mind," observes Dr Arneth, "for which the English are so generally and so deservedly celebrated, appears to guide them in the arrangement of their museums As in London, so there is scarcely an hospital of any considerable size in Dublin without one Perhaps the most beautiful is the extensive museum belonging to the Richmond Hospital, chiefly formed by Dr Robert Smith the author of the valuable treatises on neuroma and on fractures in the vicinity of joints This important collection, consisting of preparations, casts, and drawings almost exclusively taken from cases which occurred in the hospital, is exceedingly instructive" (p 196)

The obstetric museums, which are also very instructive, are that of Dr Montgomery at Sir P Dun's Hospital, and the collection at the Lying-in Hospital

> "For a city which, like Dublin, possesses a considerable number of moderate sized hospitals, the Pathological Society is a very useful association, and first

---

* For the mortality at the Dublin Hospital in later years, see note, p 369

established here, has since been imitated in many towns in Great Britain Meetings are held on stated days, and each member brings with him the preparations of such cases as he may have had under his care, and which appear to him sufficiently interesting This is attended with the advantage of explaining the symptoms which had been observed at the bed side, by the appearances presented after death, or by parts removed by operation, and of making that the property of many, which would otherwise have gone to increase the experience of but one, who, perhaps would have paid but little attention to what he saw, were it not for the opportunity of thus publicly bringing forward his observations" (p 197)

"One of the chief attractions," continues Dr Arneth, "of the Dublin School, is the great lying in hospital, which in respect to the number of deliveries is certainly inferior to that of Vienna, but about equals the Parisian Maternité, and the Prague Institution The Dublin School of Midwifery is, properly speaking, the only one of importance in Great Britain, since all the others are arranged on the principle of sending the pupils to attend women at their own houses but it need scarcely be mentioned how insufficient such a plan is and how small a number of pupils can be instructed, compared to what can be taught in a well arranged lying in hospital The advantage which Dublin thus possesses constantly attracts English and American students, who resort to it for the prosecution of their obstetric studies'

This institution is indebted for its existence to the philanthropic munificence of a private individual, Dr Mosse, who in 1745 opened the Lying-in Hospital at his sole cost

"The benefits arising from it having attracted other contributors, the first stone of the present building was laid in 1750, and in 1757 the new hospital was opened for the admission of patients Behind it is a handsome square, which in summer is open, for the benefit of the hospital, as a place of public amusement, and, singular as it may sound to a stranger's ear, the garden of the Lying in Hospital is the representative of Hyde Park, the Champs Elysees, or the Prater, in bringing together the fashionable world of Dublin" (p 199)

Dr Arneth enters into a minute description of the arrangement of the hospital, and of the mode of ventilation introduced by Dr Clarke in 1783, consisting of one or two tubes passing from the top of each ward, and opening on the roof of the house, and of a number of small openings in the doors which admit the air This plan was suggested by Dr Clarke in consequence of his supposing that the immense mortality which prevailed among the children—2944, or 1 in 6, having died of convulsions or nine-day fits in the first twenty five years after the opening of the hospital—was caused by imperfect ventilation, on the adoption of his suggestion the mortality immediately fell to 1 in $19\frac{1}{3}$, and a case of trismus is now of extremely rare occurrence

Dr Arneth remarks on the total absence from the Dublin Hospital of the "puerperal odour" so oppressive in the Maternité, and which it has been found impossible entirely to avoid in the overcrowded wards in Vienna He also alludes to the favourable state of the health in the first-named institution, compared with the almost exactly opposite condition in the Clinique at Paris, and this he attributes partly to the abundance of space, the extremely great cleanliness, the favourable situation of the house, and the excellent ventilation, and partly to the previous habits of the women, most of those in Dublin being married, while many of the patients in Paris are persons addicted to dissipation

Dr Arneth gives a very full description of the hospital, of which the following is a summary —All the wards open on a corridor To each large ward, containing on an average seven patients, two smaller ones, each having but two beds, and likewise opening on the corridor, are attached On the occurrence of any illness in the ward the patient is removed into one of these small adjoining apartments, and the communication with the large ward is, if necessary, cut off Each of the large wards becomes in turn the "labour-ward," the women being delivered on a couch provided for the purpose, and in two hours after delivery the patient is carried from the couch to her bed The women are kept in hospital for eight days after parturition, when, *if they are perfectly recovered*, they are discharged The ward then remains empty, on an average, for two days, during which time the freest ventilation is maintained, and the floors, bedsteads, &c, are scoured and washed with solution of chloride of lime The beds are composed of straw enclosed in ticken, the former is thrown out, the latter thoroughly aired, and, if necessary, washed after each patient Each ward has an ample supply of linen, which is changed very frequently, not the least uncleanliness being permitted On the roof of the house is a reservoir, which is kept, by means of a forcing pump, constantly filled with water, from this an abundant supply reaches each corridor, and is in connexion with an apparatus for the speedy and complete removal of offal

The rotation system above alluded to, in contrast with the plan of having one special labour-ward, is objected to by Dr Arneth, as the noise of those still in labour tends to interfere with the rest of the women who have been already delivered, and he says it would be quite inadmissible in Vienna, where sometimes twenty-four women lie in one ward But the advantage of having the labour-ward thoroughly and frequently cleansed and ventilated is so great, that our answer would be, that it is better to have the moderate-sized wards of the Dublin hospital, and the rotation system, than one constantly-occupied labour-ward, as in Vienna and Paris, involving, as the latter plan does, a long and often cold transit back to bed, which in Vienna is usually performed by the patient a few hours after delivery, *on foot!* (See p 32, *note*, and pp 48, 52)

In Dublin the patients are not admitted until labour has commenced This appears to us to be preferable to taking them in at the eighth month if they desire it, as is done in Vienna whenever there is room in the house, as surely a residence in hospital must be about the worst possible preparation for meeting the perils of child-birth It should be mentioned that no student who has been engaged in a post-mortem examination is allowed to enter the labour-ward on the same day that he has been so occupied, and that the assistants who, under the master, are in charge of the house by alternate months, take no part in an autopsy during their period of duty

The hospital is partly supported by a parliamentary grant, which in 1828 was 2769*l*, in 1835 was reduced to 1000*l*, and is now but 600*l*, at which last-named amount it is, we believe, to be continued to the institution as a chartered school of midwifery

"We cannot," says Dr Arneth, "pass over so large an institution without adverting to some points of comparison between it and the Vienna Lying-in Hospital' (p 204) The use of the "binder" of linen or flannel, which in Dublin is applied soon after the birth of the child, and extends from the hips to the epigastrium, is scarcely known in Vienna Its advantages are warmth, the greater ease with which it can be changed than the night-dress, by which cleanliness is more easily ensured, it is also said, by gentle pressure, to stimulate the uterus to contract, and so to facilitate the coming away of the placenta, and to render the occurrence of hæmorrhage less likely, which latter statement appears to be corroborated by a comparison of the relative frequency of hæmorrhage in Vienna and Dublin

In Dublin the child is not applied to the breast until there is a considerable secretion of milk, as it is said that before that time the nipples are easily made sore, and the infant gets no nourishment In Vienna the child is generally applied three hours after birth, without the occurrence of any bad consequences, and the suckling appears to hasten the secretion of the milk This observation, so far as Dublin is concerned, only applies to primiparous cases, or where there is a great tendency to sore nipples

1 The greatest and most important difference between the treatment employed at the two hospitals consists in the greater frequency with which perforation is used in Dublin, and the forceps in Vienna The results appear to favour the Vienna treatment, where the mortality of the mothers subjected to these operations is 1 in 6, while in Dublin it is 1 in 4 The difference in the mortality of the children is of course much more striking In Dublin, of 261 born of mothers delivered by these operations, 220 were dead, and but 41 living, while, in Vienna, of 49 children, 31 were born alive, and only 18 dead Dr Sinclair has furnished us with the following "The sum of the forceps and crotchet cases for 1850, 51, and 52, is 170, of these women, 13, or about 1 in 13, died"

2 In rigidity of the os uteri, the tepid sit bath is employed in Vienna with safety and success In Dublin, bleeding when admissible and nauseating doses of tartar-emetic when not contra indicated by the presence of gastro-enteric symptoms, are had recourse to, the warm sit-bath, which is dreaded as tending to hæmorrhage, is not used, except in cases where the other means fail

3 In prolapse of the funis, reduction with the entire hand is practised in Vienna as often as possible, when the os uteri is sufficiently dilated, the head of the child moveable, and when the funis is not in a flaccid state In Dublin, as few cases were suited for the application of the forceps or for turning, it was found most advantageous to hold up the funis diligently with the fingers, assisted with a piece of sponge This often proved useless, nor did greater success attend the plan of enclosing the funis in a little leathern bag, and pushing it back over the head The use of catheter-like instruments, ergot, stimulating enemata, &c, did not appear to be followed by better results In Vienna, of 76 children, 60 were born alive, in Dublin, of 200, only 53 were saved Every practical man will see that Dr Arneth has not paid sufficient attention to this subject, cases to admit of his mode of treatment are of very rare occurrence *

4 The treatment of puerperal fever is usually commenced in Dublin

---

* We are informed on the authority we have already quoted that the cases of prolapse of the funis in the Dublin Hospital during the last three years were 31 in number in eleven of which the cord was pulseless on admission of the remaining 20 14 were born alive version forceps and manual delivery being resorted to according to the indication The catheters, stilettes sponges and bags alluded to by the author are now at least never used and with regard to looping the funis on an extremity, it is considered expedient to turn if the hand be once introduced

with the administration of small doses of calomel, hot turpentine fomentations, poultices, and warm baths Castor-oil and turpentine are often given internally In most cases, venesection and leeching are employed Dr Arneth thinks this treatment was not more successful than the expectant plan pursued in Vienna

As in the case of London, this chapter concludes with a notice of the principal accoucheurs of Dublin, and their writings

Dr Arneth does not give a description of any medical or obstetric institution in Scotland, but thinking that he cannot better describe the state of midwifery in that kingdom than by examining the views of "the most diligent, perhaps," he says, "of all living accoucheurs, whose influence is so great throughout the whole of Great Britain," devotes the chapter on Edinburgh principally to an investigation of the opinions and writings of Dr Simpson He describes the introduction by the latter of the employment of anæsthetics, first in abnormal and subsequently in ordinary labours, and gives a *resumé* of the views of those practitioners who have written on the subject Dr Arneth alludes to many other topics contained in the writings of Dr Simpson, but as these have already appeared or been treated of in the British journals, and are but little commented on by Dr Arneth, they do not call for more special attention here One peculiarity in Scottish midwifery practice justly meets the author's reprobation—the custom, namely, which, originating with Burns and Hamilton, prevails in Edinburgh, of dilating the os uteri with the finger in certain cases during the first stage of labour In connexion with this subject he remarks, that it is singular how in some countries and schools particular doctrines, which are elsewhere almost wholly given up, are adhered to with obstinacy and prejudice, and are handed down from generation to generation Dr Arneth states, that on this subject most German accoucheurs agree with him, and he reminds his readers that many years ago a fierce contest was carried on in reference to it between the Edinburgh and Dublin schools

In his last chapter, the author institutes a comparison between the English and German midwifery practice The first subject he treats of is the use of the forceps In Great Britain and Ireland he found that most accoucheurs used the short forceps without the pelvic curvature Dr Conquest seems to have been one of the first to introduce the use of the long forceps into England Drs Simpson, Ramsbotham, &c, also make use of the so-called long forceps, but, like the Germans, only employ it when the head is either in the pelvis, or at least remains without moving on the brim The "long forceps," in the French sense, is not used by either the English or the Germans

From an examination of the writings of various authors, Dr Arneth concludes that

> —"the English accoucheurs in general—owing to the shortness and want of the pelvic curvature of their instruments, the long delay they insist on as necessary to justify instrumental interference, and the inconvenient position in which they place their patients—do not avail themselves to the full extent of the advantages offered by the forceps" (p 316)

In support of this opinion he quotes the conditions laid down by Dr Collins as necessary to warrant the application of the forceps, which, Dr Collins says, were only fulfilled in fourteen among 16,414 deliveries

Dr Arneth regrets that the definition of "protracted labours" is not fixed, and is perhaps incapable of being fixed, but he thinks that a comparison of the cases given by authors under this denomination, will show that a practitioner must use the perforator more frequently in proportion as he rejects the forceps, and that the more frequent use of the perforator than of the forceps, while it is necessarily destructive of the child, does not increase the mother's chance of life

Dr Arneth observes, that in the operation of craniotomy the English employ exclusively the scissors shaped instrument, and neither the trepan nor the cephalotribe

German and English physicians differ widely, according to the author, as to the period of pregnancy at which it is allowable to induce premature labour Most of the former insist that the life of the child is not to be wholly overlooked in anxiety for that of the mother, and that consequently the operation should not be undertaken until such time as hope might be entertained of bringing a viable child into the world—that is, that it should not be attempted before the seventh month The English, he states, on the contrary, do not attempt to save the life of the child, but induce abortion so early as the fourth or fifth month We cannot conceive how the author could have made this mistake, as Dr Merriman and all the best writers have distinctly laid down that premature labour ought not to be brought on until the seventh month, or even a fortnight later, if the case will admit of the delay *

The author attributes this supposed difference to the great and wholesome dread of the Cæsarean operation entertained by the English practitioners, who hold that this fearful operation is to be had recourse to only in cases of extreme narrowness of the pelvis

> "The indication for inducing artificial abortion is, besides, not always clear, the most experienced obstetrician will often feel doubtful whether it should be undertaken or deferred" (p 322)
>
> "It is well known that the artificial induction of premature labour was first proposed and performed in England, and that it is still adopted more frequently there than in other parts of Europe Thus Dr Ramsbotham in eleven years performed it forty times, Dr Hamilton performed it forty six times, Dr Lee in one patient induced premature labour thirteen times" (p 328)

It is, however, remarkable how much Ireland differs from England and Scotland in the frequency with which this operation is had recourse to In 16,654 deliveries which occurred at the lying-in hospital during the seven years' mastership of Dr Collins, the operation was but once performed, and more recently, among 6702 births reported by M'Clintoch and Hardy, we have only the feeble contingent of a single operation There is, however, a fallacy in this observation of Dr Arneth's, as such cases scarcely ever apply at the hospital In Ireland, too, cases of deformity of the pelvis so great as to require the induction of premature labour are of rare occurrence—which may be accounted for by the circumstance of young girls not being employed in factories

Dr Arneth makes the difference between the English and German views on the communicability of puerperal fever by contact to consist in this that the British practitioners consider that the disease, *qua talis*, may be carried by a third party from one woman to another, while the Germans are of opinion that the mode in which this communication takes place is by the conveyance of putrid matters to the genitals of the female

---

* See a Synopsis of the various kinds of Difficult Parturition By Samuel Merriman M D Fifth Edition London John Churchill 1838 p 182.

The decision of this question is, he observes, of great practical importance, for while the English, with their view of the case, will not prohibit persons who have been engaged in the post-mortem examination of the bodies of those who have died of other than puerperal affections, from attending midwifery cases immediately after, the Germans will not hesitate to go from a patient labouring under puerperal disease to visit another in her confinement, without changing their clothes and observing the precautions prescribed by the English The author states, too, that in Vienna cases of erysipelas have not been observed to be more frequent during epidemics of puerperal fever than at other times He will not deny, however, that diseases may differ in their properties according to the countries in which they occur—as in Germany, typhus is usually attended with ulceration of the small intestine, while in England (and more particularly Ireland), where this ulceration is rarely found, the exanthematous form of the disease prevails

In this country, practitioners of midwifery seldom themselves perform post-mortem examinations, so that here the disease is not likely to arise from the reception of an animal poison from the hand of the accoucheur

The author quite agrees with English practitioners in their great aversion to the Cæsarean operation, and thinks, with them, that it should, generally speaking, be limited to cases of extreme narrowness of the pelvis, but at the same time quotes Dr Shekleton's case of pelvic tumour, published in the 'Dublin Quarterly Journal' for November, 1850, and Dr Oldham's successful performance of the operation in a case of cancer of the cervix uteri, which appeared in the number of this journal for April, 1852, as proofs that other circumstances than deformity of the bones of the pelvis may indicate the operation

As we premised in the commencement of the foregoing examination of Dr Arneth's work, our principal object has been to ascertain and bring before our readers the impressions made upon the author by our institutions, and by the views of the profession in this country In this we felt we should best succeed by drawing up a faithful summary of his observations, but as his volume embraces a great variety of subjects, our sketch must necessarily be imperfect, and we have been obliged to pass superficially over many points of practical importance, while to some interesting subjects, such as the Foundling Institutions in France and Austria, we have not at all adverted Enough has, however, been given, to show the value of the book, which is evidently the result of minute and careful observation we consider it to be a favourable specimen of a most useful class of publications, and we congratulate the author on possessing that ardour in investigation, which is the best element of success, and which alone could enable him in so brief a period of time to acquire the amount of knowledge he has proved himself to possess of the institutions of the countries he has visited, as well as of the opinions of the profession in each Had his stay among us been of longer duration, he would doubtless have avoided some inaccuracies into which he has fallen, and which it has been our duty to point out

William D Moore

REVIEW VII

1 *Archiv des Vereins fur gemeinschaftliche Arbeiten zur Forderung der wissenschaftlichen Heilkunde* Herausgegeben von Dr J VOGEL, Dr H NASSE, und Dr F W BENEKE Bd I, Heft 1 1853

Archives of the Society for United Labours towards the Advancement of Scientific Medicine Edited by Drs VOGEL, NASSE, and BENEKE

2 *Correspondenz-Blatt des Vereins fur gemeinschaftliche Arbeiten zur Forderung der wissenschaftlichen Heilkunde* Nos I to III

Corresponding-Sheet of the Society for United Labours towards the Advancement of Scientific Medicine

THIS new German journal is the organ of a society which has been lately founded in Germany by some of the most active labourers in the field of pathology, and which has for its special purpose the investigation of disease by the combined efforts of many observers working towards a given end It is singular enough, that lately, in this country, several societies have been originated to carry out the same object,* and it would therefore appear that the need has been felt, both in Germany and in England, of extending the field of observation beyond the narrow limits to which an individual is necessarily restricted, and of bringing to bear on the elucidation of the complex phenomena of disease, the mighty force derived from intellectual combination

As the action of societies of this kind differs necessarily in some respects from that contemplated in the formation of the older medical societies, it will be useful to inquire exactly what their founders wish to do, and how they intend to do it In doing this we shall take for our text the works quoted at the head of our article

The objects of the German society are laid down by Professor Vogel, in a very interesting paper, entitled 'What we Wish,' which opens the journal before us They are as follow

1 *To give mutual impulse to scientific researches by personal and written communications* —On this object it is not necessary to comment

2 *To originate and advance works which otherwise would not appear at all, or not in the same shape* —The society will, for example, prompt an observer to a special course of investigation, will regulate the direction he should take, and prevent him from wasting his time and strength in abortive efforts In the case of young men especially, who often have much leisure time and facility of investigation, but who want a knowledge of the exact course it is most expedient for them to pursue, such advice and aid as is here contemplated will be very valuable Young men often throw away a vast amount of precious time and zeal in searching after shadows, and finding no substance, they abandon the path of observation

---

* For instance the London Medical Society of Observation which has lately published its method of clinical and post-mortem examination under the title What to Observe at the Bedside and After Death in Medical Cases' In the laws of this society one object is stated to be to exhibit the special advantages which may accrue to the science of medicine by the co operation of several persons working on a uniform plan towards the elucidation of given medical questions' The Epidemiological Society and a society organized by Dr Fleming for the purpose of investigating therapeutical problems, are also examples

in despair. To use an image in 'Faust,' they dig for treasures, but they merely scratch the surface, and find earthworms. But under the direction of an older head, young men, scarce entered upon life, have done great things for science. It is certainly extraordinary how much of the progress of medical science of late years can be traced to those inaugural dissertations which, under the auspices of Bidder, Lehmann, Vogel, or Liebig, have issued from the laboratories of Dorpat and Giessen.

The society also contemplates the arrangement by its members of the various private and isolated memoranda which most practical men have accumulated, and of the many elaborate details of pathology which now lie buried and unproductive in the case-books of hospitals.

3. *To consider and determine problems to be investigated, as well as the necessary means to be adopted for their solution.*—This is the most important and vital object of the society, and may be thus expressed. The society will indicate to its members certain problems which it desires to see worked out, and it will also intimate the mode in which this is to be done. The necessity of arranging the method of investigation is evident; for if the observations of several hundred physicians are to be analyzed, they must have been conducted according to the same plan, otherwise comparison becomes impossible. If, for example, the problem be submitted, "What is the specific gravity of the urine or the blood?" it is of course clear that the specific gravity must be taken by one method only, and with reference to certain specified conditions.

4. *To lighten and facilitate the labour of research by division of, and assistance in, the work.*—The advantage, and indeed necessity, of examining physiological and pathological problems by the aid of various methods, with all of which a single observer cannot be familiar, is obvious. Many of the most important works in physiology have been compounded of the conjoint labours of a pure physiologist and a professional chemist; we need hardly refer to the classic work of Tiedemann and Gmelin, or to the more recent but not less valuable addition to knowledge which Bidder and Schmidt have given to the world. In pathology, unfortunately, we have as yet no good instance of this division of labour; but if it were possible to bring together the efforts of the clinical physician, the statician, the microscopist and the chemist, no disease would long remain a mystery. As the modes of investigation in pathology acquire order and form, this co-operation will become easier; and it is for this reason, as well as for others equally important, that we regard as of the greatest moment a movement which tends to render precise and uniform the method in which the deviations from health are observed and recorded.

5. In this way it is hoped that the society will form, as it were, a committee of reference, which may accomplish the solution of such problems as not only surpass the powers of a single observer, but cannot even be solved by the additional help which this observer might be able to obtain for himself. In such a case he can fall back on the society, and submit the difficult point for their consideration.

The problems which the society propose for investigation, may refer, of course, to any department of medicine, to diagnosis, or to therapeutics, to surgery or midwifery, to ophthalmology or otology, to pathological anatomy or pathological chemistry. But the chief directions in which, at the present moment, investigation is needed, appear to Professor Vogel to be the following

(*a*) Investigations on the metamorphosis of matter in health and disease, the study of excretions (urine, fæces, sweat) of the blood, of the influence of food, &c, present problems which will occupy hundreds of the most diligent investigators for many years

(*b*) Anatomico-physical investigations (conducted in living and dead bodies) on the normal size and weight of the entire body and its organs, and the changes in them which can be discovered by auscultation, percussion, mensuration, spirometry, &c

(*c*) Investigations on the relative frequency of certain diseases in different places, on the causes of disease, the influence of climate, occupation, &c

(*d*) Accurate and numerous experiments on the action of various articles of food and medicine on healthy and diseased persons

(*e*) Clinical observations of cases of disease, and the subsequent working up of these into form

Without limiting itself precisely to these directions, the society will yet bestow its chief strength upon them, as the problems stated under the above heads are evidently of the greatest immediate importance for practical medicine

Such being the objects of the society, let us see how they intend to carry them out The society now consists of about 200 German, some ten or twelve English, and a few French members Among the former appear the names of many of the most celebrated physicians of Northern Germany, such as Wagner, Henle, Virchow, Moleschott, Schlossberger, &c, and a great number of the younger men, such as Panum, Hofle, &c, from whom science has most to expect in the future The business is carried on by local secretaries, and by Professors Vogel and Nasse, the editors in common with Dr Beneke, the general secretary, of the 'Archiv' The annual subscription amounts to some four to eight English shillings, and for this sum each member receives the 'Corresponding Sheet,' which is issued every month or six weeks, and gives an account of the progress of the society, the problems which are to be worked out, and the methods of doing so From time to time the 'Archiv' is published, and paid for separately It contains the results of the inquiries, and such papers as individual members may wish to insert, and which are approved of by the editors

The first thing which the society obviously had to do, was to arrange their general methods of examination The first number therefore, of the 'Archiv' contains an elaborate paper by Dr Beneke, on the mode of examining patients, and a second paper, by Professor Vogel, on the means of clinically determining the amount and rapidity of the metamorphosis of tissue, especially by the examination of the urine

We shall not enter into any critical examination of these two chapters, as we prefer to allow Professor Vogel to state his own views in a review of the work lately published by the London Society of Observation, entitled, 'What to observe,' which we shall insert in our next number The 'Archiv' contains, also, a paper by Dr. Conradi, bearing on clinical examination—viz., the modes of discovering the size and position of the lungs, heart, liver, and spleen, in which some new and important information is given, for which we refer to the 'Chronicle'

In thus fixing its mode of examination, the society is fully aware that the subject is not yet nearly exhausted, and it is prepared to adopt any suggestions, and to make any improvements, which its members may from time to time urge on its attention In this it will wisely follow the example of the cognate sciences, such as chemistry and astronomy, which have equally a common general method, in which alterations and improvements are constantly being made

It remains now only to state what the society has already commenced to do The first 'Corresponding Sheet contains a statement of the weights and measures which are to be used, and formulæ for reducing others into these The society is then invited to prove the accuracy of the modes now frequently adopted for the quantitative examination of the urine by the addition of measured quantities of reagents of known strength This method (*Titrirmethoden*), if accurate, will be most valuable, as in two or three hours, instead of twice as many days, almost all the constituents of the urine can be quantitatively determined Thus the amount of the phosphoric acid is known by the addition of measured quantities of chloride of iron, by which the phosphoric acid is thrown down as phosphate (Breed), the chlorine and urea are determined by the pernitrate of mercury (Liebig), &c This is the first problem, and the most important one yet proposed

The second problem is to determine the quantity of the urine, and of the solids contained in it, of healthy and diseased individuals at different times of the day, in relation to meals, bodily exercise, intestinal excreta, atmospheric conditions

The third problem is to determine the quantity of ozone in the air, for the purpose of finally seeing whether or not the opinions of Schonbein have any foundation

The fourth, fifth, and sixth problems are the determination of the frequency of the pulse at various times in the twenty four hours, and the determination of the variations of temperature and weight in the same time A scheme given for the investigation of the second problem answers for these also

In the second and third 'Corresponding Sheet some other problems are proposed, of which we extract only two or three specimens

> "The observation that in pneumonia and pleuro pneumonia albumen not seldom appears in the urine, and that pneumonias thus characterized are not seldom antecedents or the commencements of tuberculous exudations in the lungs or of chronic affections of other organs, make the answer of the following questions useful In what conditions of the individual, and in what relation to the whole number of cases of pneumonia, does albuminuria occur in the beginning of the disease? Has it a prognostic value? How did the case of pneumonia proceed? What treatment was employed, and with what success?"

Another problem is the exact diagnostic value of dilatation of the pupil in disease.

Another is thus given in the form of a question to the members.

> "Does dysentery occur in your district? Sporadic? Epidemic? When? What is the number of cases in relation to the other sick, and to the population? Mortality?"

We have now said enough to give a general idea of the objects and action of this society While in this country the Epidemiological Society, in its own department, proceeds somewhat upon the same plan, and while the London Society of Observation seeks by a different organization to attain somewhat similar ends, we must admit that by extending its sphere of observation, the German Clinical Society (to give them a short title) have advanced still more this great cause of co operation in science When once they get into working order, there is hardly any problem they cannot easily investigate Let any one picture to himself the labour it would cost him to determine accurately, and with reference to time of year, temperature, exercise, weight of body, kind and quantity of food, &c, the quantity of phosphoric or uric acid in the urine of a healthy man for twenty-four hours Propose this problem to the society, and, with comparatively no labour at all, each member could furnish one or two observations The collective observations would amount to two hundred or four hundred experiments, a number which it would take a single observer more than a year to collect Or supposing that, in order to answer the question on the dilatation of the pupil, each member observes 100 patients, which he might very well do in three or four weeks, the society will have no less than 20,000 observations to arrange, and to deduce conclusions from These considerations, simple as they are, strike the mind almost with wonder that scientific co operation has not long ago been carried out more systematically and fully

We need not occupy further space by adducing additional examples, or to show how easy it will be for the chemist and the anatomist to combine their modes of exploration We think it will be admitted that the advantages which will result if the society can successfully carry out its plans, are great and manifold Of course there are difficulties in the way, and good generalship will be required in directing the course of this scientific army Perhaps in no other country but Germany could such a plan be promulgated with any chance of success In Germany, however, the standard of education is now so high, and the members of the profession appear as a general rule to be so animated with true zeal for the advancement of scientific medicine, that a system which demands considerable labour, skill, and enthusiasm, and which will also obviously tend more to advance science than to make individual reputations (since personal labour will, to a certain extent, be merged in the general result), will probably be carried out It will also, we believe, meet with sympathy in this country, and in America In both countries, the old maxim that union is strength, is a household word, and it is fully known to be as applicable to science as to meaner things

We may remark, that this German society wishes to include in its ranks men of all nations, and that it has already some English members and a secretary in London * The more labourers, the sooner will the problems now submitted be answered, and fresh ones be proposed We cannot too strongly urge upon our fellow-countrymen who wish to see our art rendered more useful even than it is, to join in this excellent undertaking The general practitioner most occupied with practice would find some time to answer at any rate two or three of the problems,

---

* The secretary, Dr Hermann Weber, 49, Finsbury square, will answer any inquiries respecting the Society

in the course of the year Without neglecting his English societies, and the subjects he may individually wish to investigate, he might contribute his small share of labour towards the grand total It is by the union of such small and apparently insignificant efforts, that the great force is constituted, which cleaves the rock and sets free the living waters As the society has appointed a local secretary, the means exist of organizing both English and American corresponding societies on such a scale as to permit the 'Corresponding Sheets' and schemes, to be translated into English There is so much sympathy already between the German and the English characters, that we cannot doubt the ease with which harmonious co operation could be carried on If this can be done, the generation now engaged in the practice of medicine will leave to that which is to follow them a very different position than they inherited from their predecessors

E A Parkes

REVIEW VIII

1 *Traité pratique des Maladies Vénériennes, contenant un chapitre sur la Syphilisation, &c* Par S G MAISONNEUVE, M D, et H MONTANIER, M D —*Paris*, 1853 pp 564

Practical Treatise on Venereal Diseases, with a chapter on Syphilization

2 *Traité des Maladies Vénériennes* Par A VIDAL (DE CASSIS) —*Paris*, 1853 pp 549

Treatise on Venereal Diseases

3 *Report à M le Préfet de Police sur la question de savoir si M le Dr Auzias Turenne peut être autorisé à appliquer ou à expérimenter la Syphilisation, à l'Infirmerie de la Prison St Lazare?* Par MM les Docteurs MELIER, PHILIPPE RICORD, DENIS, CONNEAU, et MARCHAL (DE CALVI)

Report to the Prefect of Police, on the question whether Dr Auzias Turenne be permitted to practise or experiment on Syphilization, in the Infirmary of the Prison St Lazare?

4 *Syphilitic Diseases, their Pathology, Diagnosis, and Treatment, including Experimental Researches on Inoculation, as a differential agent in testing the character of these affections* By JOHN EGAN, M D, M R I A —*London*, 1853 pp 346

5 *Observations on Syphilis, and on Inoculation as a means of Diagnosis* By JOHN CROWCH CHRISTOPHERS, M R C S —*London*, 1853 pp 74

6 *Traité pratique des Maladies Vénériennes* Par le Dr PHILIPPE RICORD —*Paris*, 1851

7 *La Sifilizzazione studiata qual mezzo curativo e preservativo delle Malattie Veneree* De CASIMIRO SPERINO —*Turin*, 1853 pp 903

Syphilization treated as a Curative Means in the Venereal Disease

WHENEVER many remedies are recommended for one disease, there is presumptive evidence that none of them are of much value If any one could be relied upon, the others would soon cease to be used, and fall into disrepute It is the fact of the medicines employed not answering all that is required of them which opens the way for the trial of new remedies, these, in their turn, sustain their reputation until a sufficiently extended experience indicates the limit of their operations

So is it with the medical theories upon which our practice is founded If the symptoms observed in any disease are explained on many different hypotheses, we have presumptive evidence that its true nature is not thoroughly understood It is the want of accordance of some of the symptoms observed with the existing theories of the nature of the disease which continually calls forth fresh explanations These, on their first appearance, are generally made to account for several dissimilar actions, and an undue importance is attached to them, until their real value is ascertained by observation and experience

The nature and treatment of syphilis, when judged of in this way from the works which continue to appear, is far from presenting a subject upon which nothing more is to be desired The variety of views which have been maintained, the lengthened discussions which have taken place, and even the varied results of the experiments which have been recorded of late, all tend to show that the simplicity and unity so characteristic of truth, when once clearly perceived, are still wanted in the labours of those who have undertaken the elucidation of this subject

When conflicting opinions obtain with regard to a disease, the only way to arrive at a satisfactory conclusion concerning its nature is to trace, as far as may be, the morbid processes by which it is developed In as far as we can succeed in doing this, we have positive knowledge to guide us all beyond is speculation A clear line may at least thus be drawn between that which we know and that which we do not

In the following article we propose to review the different theories of the nature of syphilis which have been lately promulgated on the continent and in this country, and we shall endeavour to bring the various views entertained to the above-mentioned test An opportunity will thus be afforded of ascertaining how far the discordant evidence which they appear to present may be reconciled, and of distinguishing that which is demonstrated from that which is ideal

To M Ricord is undoubtedly due the credit of having introduced a new method of investigating the nature of the venereal diseases—namely, that of inoculating the products of the different affections which arise, with the point of a lancet, on some apparently healthy part of the skin of the affected person

By this method of investigation, M Ricord arrives at the following conclusions, which are adopted by MM Maisonneuve and Montanier

1 That blennorrhagia and chancre are two affections entirely distinct
2 That blennorrhagia is an inflammatory, contagious affection.
3 That a chancre always produces a specific virus
4 That blennorrhagia never gives rise to constitutional syphilis
5 That a chancre always produces a constitutional disease when it becomes indurated
6 That a non-indurated chancre is always a local disease, and is never accompanied or followed by secondary affections
7 That a chancre is the only origin of syphilis

8 That pus derived from a primary affection is the only contagious element in the disease It may be secreted by the skin, by a lymphatic vessel or gland, and the ulcer which supplies it may be simple, indurated, or phagedænic
9 That no secondary or tertiary disease is capable of being communicated by contagion
10 That blennorrhagia may arise spontaneously, and never produces any disease but blennorrhagia
11 That a chancre is always and of necessity the result of a chancre
12 That there is but one syphilitic virus'

These conclusions are the results of experiments said to have been repeated a thousand times, under a great variety of circumstances In the experiments made by M Ricord, he found that simple blennorrhagia never gave rise to a chancre, and he therefore concludes that this disease is not syphilitic If the discharge, however, arises from a concealed chancre in the urethra, then inoculation may produce a chancre, this affection M Ricord considers syphilitic He believes the pus from a chancre to be always capable of being inoculated when taken during its period of progress, or during the time that it remains stationary, but not when taken during the period of its reparation, it has then become a harmless ulcer The pus from a bubo he likewise regards as inoculable when taken from the gland itself, but not when derived from the surrounding cellular tissue

By this method of investigation, M Ricord has endeavoured to draw a clear and well marked distinction between those diseases which are syphilitic in their origin and those which are not As general conclusions from all his experiments, he deduces—

> That a primary venereal sore which has healed, or still remains open, does not prevent other similar sores from forming, and that there appears to be no definite limit to the number of successive inoculations which may take place
>
> That a person actually infected with primary disease in one part is never subject to the occurrence of similar affections elsewhere, unless the disease be conveyed by the contact of the matter from the primary sore, or be communicated in like manner from some other person
>
> That secondary disease depending upon a general infection of the system will never prevent a person from contracting fresh primary sores
>
> That the frequency with which constitutional symptoms manifest themselves is by no means in proportion to the number of primary sores developed at the same time

We have thus been particular in giving an accurate outline of M Ricord's doctrines, as upon them have been in a great measure based the discussions which have lately so much agitated the scientific meetings of the profession on the Continent Upon these propositions we shall now make some remarks, and endeavour to bring the principal of them to the test which has been proposed

The concurrent testimony of all observers has now ceded to M Ricord his first point—namely, that the pus derived from a chancre contains a very powerful agent which escapes detection by our senses, and which is not found in the pus of blennorrhagia, when this subtle and occult agent is inoculated, the morbid process which ensues may be traced in the most satisfactory manner. If the pus from a chancre during its period of progress, or during the time that it remains stationary, be inoculated, the following succession of effects may be observed.

> "During the first twenty four hours, some redness appears at the spot where the inoculation has been made. From the second to the third day there is some swelling affording the appearance of a pimple surrounded by a red areola. From the third to the fourth day, the cuticle is raised by a more or less turbid fluid, which often forms a vesicle with a little dark spot in its centre. This spot results from a small effusion of blood at the time of the inoculation. About the fourth or fifth day the secretion becomes increased in quantity, and more or less purulent in appearance. The centre of the pustule now becomes depressed, in this respect resembling the pustules of small pox; at the same time the surrounding redness may become fainter. From the fifth day the subjacent tissue, which had before undergone no change beyond being slightly œdematous, becomes infiltrated by a plastic lymph, which affords to the touch the peculiar hard and elastic feeling of certain forms of cartilage. After the sixth day the pus becomes thicker, the cuticle over it gives way, and scabs form on the surface of the part. When these scabs are detached, a chancre presents itself below characterized by its indurated base, involving the whole thickness of the skin, by its greyish white surface, composed of a lardaceous matter, or of a false membrane, and by its sharp and undermined edges, having generally a circular form." (Maisonneuve and Montanier, pp. 14, 15.)

Nothing similar to this is ever observed when the pus inoculated is derived from a simple case of blennorrhagia. M. Ricord and his school believe that these results *always* follow inoculation from an indurated venereal ulcer; and Dr. Egan, although differing, as we shall see, on other points, affirms "that in the indurated and excavated form, inoculation supplies a valuable and unerring test, the characteristic pustule being *always* the result of the operation." (p. 39.)

M. Vidal, the latest writer upon the opposite side, is of opinion that certain persons are naturally incapable of being inoculated with the syphilitic virus, and that in others this immunity may be acquired under certain circumstances—as an instance of the first kind, he cites the case of a man well known to many of those who had taken part in the discussion on syphilization in the Academy of Medicine at Paris, (p. 47.) This person had never been affected with syphilis, although he had continually exposed himself to contagion. He was, moreover, twice inoculated with pus taken from a chancre during its active stage, with no result whatever. It may be said that this natural immunity supposed to be enjoyed by certain persons from the effects of inoculation, does not invalidate this means of ascertaining the real nature of venereal sores, inasmuch as in *their* case the very same conditions would render this test unnecessary.

"But," says M. Vidal, "this disposition may be acquired," and he instances the case of M. Laval, who, after having been repeatedly inoculated on the upper extremities, at length became proof against any further inoculation. "I am perfectly certain," he says, "that M. Laval was inoculated upon three occasions by M. Gosselin (with pus which produced chancres upon another patient) without effect." This same M. Laval, however, was at length successfully inoculated by M. Ricord. This is explained by M Vidal, upon the supposition that after a certain number of inoculations, the system loses its susceptibility to the effects of the syphilitic virus, for a time, but that after an interval of rest, it may again, as before, become subject to the disease

With this explanation, M Ricord and his school appear little satisfied "Give me," says he, "a person who has been syphilized, and who is proof against further infection, who will come before the class at the Hopital du Midi, or before the Academie, who will enter the lists with me and defy me with the arms of my choice" This challenge was published on the 12th of August, on the 22nd M Auzias accepts it, on the 23rd of September M Ricord declares himself ready, on the 4th of November he announces that the experiments have commenced, and that the results will be published in the 'Union Medicale'

"Search the records" cries M Malgaigne before the Academie de Medicine, "the promised communication has never arrived A week afterwards M Ricord presents M Laval to the Chirurgical Society and on the 18th of November to the Academy of Medicine Not a word is said about the above mentioned experiments, On the 9th of December M Marchal writes to the 'Gazette des Hopitaux,' that M Laval has presented himself to M Ricord who had twice made seven inoculations with three different kinds of pus, each of acknowledged efficacy without producing any result M Ricord answers nothing, and the 'Union Medicale' is silent'

M Malgaigne continues

This silence appears to me very like a defeat I should, however, have preferred an open and public avowal of the result of the experiments, and therefore I urged some explanation upon the point Several presented themselves But what did M Ricord himself say? That he had produced upon M Laval an ecthymatous pustule sufficiently characteristic not to require further proof and that the other inoculations which had failed had been done with pus, which had also failed to produce its specific effects when inoculated on the patients from whom it was taken

"No other details of these experiments are given M Ricord declares that his dignity would be hurt if he gave any further explanation That does not satisfy me,' says M Malgaigne, "what connexion is there between M Ricord's dignity and the details of an experiment? By his own avowal upon his own ground, having entered the lists with the 'arms of his choice,' M Ricord succeeded only once in seven times But the inoculations failed upon other patients which proved that the matter was not good! Who would have thought that M Ricord, after so solemn a challenge in his immense practice, and with his choice of arms, should not have been able to find some good pus?

"But at length one inoculation succeeded Granted! provided that the counter experiment which M Ricord himself declares always indispensable, was made viz, that the pus produced by the inoculation can be again inoculated

"For my part," concludes M Malgaigne "I regard the fact that certain persons may acquire an immunity against syphilitic inoculation is incontestably demonstrated If M Ricord doubts it I will engage to produce a young man who believes himself syphilized and who will defy M Ricord to produce in him a single atom of pus capable of being again inoculated M Ricord shall take his precautions If he does not succeed he shall begin again my patient declares himself ready to allow twelve hundred inoculations to be made upon him and more, should this not be deemed sufficient" (Vidal, pp 50, 51)

Leaving these chivalrous opponents for the present, we come to the consideration of M Ricord's proposition, "that a chancre is the only origin of syphilis" A very broad line of distinction is here drawn by M Ricord and his followers, between a chancre and every other form of venereal affection Primary syphilis is alone, it is said, capable of being inoculated, secondary affections never Persons may be exposed to the contact of mucous tubercles, to secondary ulcerations, to all sorts of syphilitic eruptions, and will never become infected The pus produced from these sources will never produce the characteristic pustule of syphilitic inoculation In cases where apparent exceptions have occurred, either the mucous tubercles have become transformed into real chancres by fresh contact with matter from a primary ulceration, or else real primary sores have been mistaken for some form of secondary affection "Secondary syphilis is not contagious Reason would therefore lead us, *a priori*, to infer that it is not capable of being inoculated, and experience confirms this view"*

On the other hand, M Vidal, Dr Waller of Prague, and others, maintain that mucous tubercles and secondary symptoms may, under certain circumstances, be communicated from one individual to another They dwell particularly upon the undeniable fact that a child may be infected in its mother's womb They consider it well established that a woman who has neither a chancre, nor a syphilitic bubo, but whose constitution is affected, may infect her offspring That she may bring into the world syphilitic children who will infect their nurses, who will again in their turn transmit the disease to their families

"Further," says M Vidal, "that mucous tubercles (see Pustule plate) may be transmitted, is generally acknowledged On this point MM Lagneau, Baumes, Cazenave, Gibert, and all observers whose judgment is not warped, may be consulted" (pp 55, 56)

M Ricord, however, maintains that the origin of the mucous tubercles (said by these gentlemen to have been transmitted) has not been seen, and that the evidence afforded by the neighbouring glands has not been taken into the account A specific ulcer may at first have been the real disease, and have become subsequently transformed into a mucous tubercle, and he believes that all syphilitic disease said to be communicated by contact with table utensils, with pipes, razors, masks, &c, have no other origin than the pus derived from a primary sore †

With regard to the transmission of the disease from mother to child, and from the child to the nurse, M Ricord notices the poverty of the evidence which we have upon the subject

> "I have seen," he says, "nurses and the children they were suckling, diseased, and both parties accused of infecting the other Generally, I have been able to trace the origin of the disease to a primary sore in one of the parties When this has not been the case, I was called too late, five or six months, perhaps, after the child had been with its nurse
>
> "I have for many years had a number of nurses at the Hopital du Midi, and I have often given them children to suckle, which were sent to me from the *Maternité*, with secondary affections Never, as far as my observation extended, were these nurses infected
>
> "On the other hand, nurses palpably affected with secondary disease have been able to suckle infants which were sent as being affected with syphilis (but who only had had simple eruptions of eczema, impetigo, or porrigo), and never were these children, whilst under my inspection, infected" (pp 100, 101)

---

* Maisonneuve and Montanier p 24

† Lettres sur la Syphilis p 98

Thus M Ricord and his followers, while they admit that a child may be affected independently of inoculation *in utero*, believe, that after the child is born, it can only be infected by contracting a primary ulcer, and that this may happen by some of the virus being carried by the fingers of the nurse, either directly to the child, or first to the nurse's breast, where a chancre is formed, and that from this the child is inoculated But M Vidal maintains, that mucous tubercles, and some forms of secondary affections, may be transmitted quite independent of the mode of inoculation which we have hitherto considered He draws a distinction between contagion in itself and particular means of contagion, and believes that syphilis may be communicated even when it cannot be inoculated, so as to produce the "characteristic pustule" Even this test he expresses himself willing to offer for the "satisfaction of the schools,' and distinctly affirms that he has succeeded in inoculating the secondary ecthymatous pustule (p 57)

M Vidal further believes that certain non-syphilitic affections which are at present considered incapable of being inoculated, might ultimately be found to be so, if the proper moment when they contained inoculable matter were taken for the experiment

> "In the attempts" he says, "which have hitherto been made to inoculate cancer, the ichorous discharge on the surface of the ulcers has been taken, or else the *débris* of the cancerous matter But are these the products with which the experiments should be tried? ought they not rather to have been performed with the cancer cell in the early stage of its development? In an experiment performed in this manner by Langenbeck, where fresh cells from a cancer still warm were introduced into the bloodvessels of a dog, cancerous tubercles were formed in the animal's lungs" (Enclycopedie Anatomique, t iv p 279)

From these and similar facts, M Vidal argues, that inoculation which fails in one way may succeed in another The mode of application may have a very material influence upon the result He instances the fact that the ordinary mode of inoculating syphilitic matter upon animals is not sufficient, and that M Auzias Turenne found that he succeeded better by removing the cuticle by means of a pair of curved scissors, and keeping the morbid matter for a minute in contact with the excoriated surface, whilst the surrounding part was gently rubbed In accordance with these views, he maintains, that although the secretion from a mucous tubercle is incapable of being inoculated in the same way as the pus from a chancre, yet that it is nevertheless capable of producing both primary and secondary venereal affections that if, for instance, a blister be applied to a portion of skin, and the excoriated skin be then dressed with lint soaked in the secretion of a mucous tubercle, the inoculation will succeed, although this same secretion may not be capable of being inoculated by a simple puncture with a lancet

> "It is probable also, that a simple inoculation with a lancet charged with syphilitic blood would produce no positive results But if according to the experiments of Waller of Prague the skin be first scarified and the infected blood be introduced with an appropriate instrument into each scarification, there is a good chance that the experiment will succeed, as it did in Waller's hands' (Vidal, p 60)

Waller's experiments are so much to the point, and of so much importance, that they cannot be overlooked in the investigation of this subject Among others he relates the following

> "The skin of a boy who had never had syphilis was scarified with a new scalpel Some blood was then drawn by means of a cupping glass from a patient affected with secondary syphilis and inserted into the scarifications on the boy's skin by means of a piece of wood Some lint soaked in the same blood was placed over the scarifications and allowed to remain No immediate inflammation or suppuration followed this operation, and at the expiration of three days the wounds had all healed, but thirty four days after this experiment, two distinct tubercles
> inoculated part These were the size of a pea, of a pa
> unattended by suppuration, pain or itching On the following day, these tubercles increased in size, so that they became united at their bases They were scaly upon the surface, and surrounded by a deep red areola They gradually increased in size, and assumed a copper hue The skin and subjacent cellular tissue which formed the base of the tubercles became hard and dense, while their surface ulcerated, and was covered by a thin brown crust
>
> "A fortnight after this, an ulcer was developed, with a base as large as a pigeon's egg, surrounded by a red, copper coloured circle, and covered by a crust similar to that above mentioned A single tubercle, the size of a pea, now formed on the right shoulder, the surface of this was red, and covered with scales After the lapse of another fortnight, the patient became feverish and restless
>
> "A month after the appearance of the first tubercle, an eruption appeared over the lower part of the stomach on the back, on the chest, and on the thighs This eruption gradually covered the whole body, and was accompanied by neither itching, pain, nor fever"

Dr Waller gives the names of several eminent men who recognised this eruption as syphilitic He concludes that—

> "Primary and secondary syphilis are capable of inoculation The pus of a chancre produces a chancre The products of secondary affections give rise to secondary affections"*

Of what practical value, then, amongst this conflict of opinions, are we to consider syphilitic inoculation? As a means of diagnosis, we must allow that when the characteristic pustule is produced, it affords satisfactory evidence of the nature of the disease This mode of ascertaining the nature of the affection we have to deal with, may be of much service where ulcers have appeared in unusual situations, and where, from this or other causes, they have not assumed their common appearances, and to M Ricord we would accord the full credit of this discovery

But can we with equal certainty say, that when inoculation fails, the disease is not syphilitic? Certainly not With the amount of evidence before us, we cannot regard the broad distinctions sought to be established by M Ricord and his followers as correct To affirm that all primary chancres can be inoculated because they are syphilitic, and that they are syphilitic because they can be inoculated, is simply to beg the whole question

Experience proves that we cannot rely upon the negative results of inoculation, and that a primary affection, although not capable of being inoculated in the ordinary way, may nevertheless be followed by secondary symptoms During the last year, the reviewer had under his care two patients in the Lock Hospital one of these had a serpiginous sore, extending from the groin, between the thighs, and around the nates; the other had a rapidly-extending phagedænic sore upon the penis, yielding an abundant purulent secretion. Both patients were inoculated. In the first case, the result was positive, and in the latter negative. The first patient continued under observation for nearly twelve months, but no secondary form of disease showed itself; the second patient had a very full crop of pustular eruption in the course of a few weeks.

---

* Du caractère contagieuse de la syphilis secondaire par Dr Waller, traduit par M Axenfeld (Annales des Maladies de la Peau et de la Syphilis, Avril, 1851

In these experiments the ulcers were of the non-indurated class, in reference to which we have the concurrent testimony of all writers, that the effects of inoculation are not so satisfactory as in indurated sores.

What, then, is the sum of positive knowledge which we derive from the practice of inoculation as a means of diagnosis? It furnishes us with a ready means of testing the nature of an indurated ulcer, and it will often (when the results of the inoculation are positive) enable us to ascertain the nature of other ulcerations. But upon the all important questions which continually present themselves to us in practice—namely, Is such a sore likely to be followed by secondary symptoms? or, Will any particular mode of treatment in reference to it prevent secondary affections? inoculation does not pretend to decide; upon these practical points it has hitherto afforded us no information.

But it is maintained by Dr. Sperino, M. Auzias Turenne, and others, that inoculation may not only serve as a means of diagnosis, but also as a means of preserving the system from the action of the syphilitic poison, or of curing the syphilitic disease when the constitution has become infected by it.

This mode of practice is not likely to be seriously entertained in England, and the facts hitherto published with regard to it require confirmation. They cannot, however, be overlooked, and have, at least, this effect, that they tend to disabuse the minds of professional men, and through them of the public, of an idea that has very generally prevailed—namely, that the severity of the constitutional symptoms, and the frequency with which they occur, bear some proportion to the number of primary sores. From Dr. Sperino's book it would appear that in no case were the inoculations which he practised followed with any serious results—a slight surrounding inflammation, or a transient phagedænic ulceration are the sum of the much dreaded consequences which are said to have followed. Nor can it be said—if his book at all corresponds to his motto, "*non verbis sed factis*"—that he has not given the plan a thorough trial. If very serious results were likely to happen, Dr. Sperino certainly ought to have met with such, for he has been by no means sparing in the number of his inoculations.

Out of 53 cases of primary syphilis, we have 52 treated by repeated syphilization. In these the plan is said to have been successful in 50, and unsuccessful in 2 cases. Out of 43 patients affected with constitutional syphilis, 26 were treated by syphilization, and 25 were cured. In 6 instances the iodide of potassium was used in conjunction with syphilization, and in 8 cases, syphilization, iodide of potassium, and mercury, were all employed. In 3 cases it was found necessary to discontinue the inoculations, and 2 patients died.

Of the 53 patients treated for primary syphilis, only 3 presented themselves subsequently with secondary affections, and in these the disease is said to have been cured by fresh syphilitic inoculations. With reference to the comparative frequency with which secondary affections may be expected after this plan of treatment, we cannot tell how much the popularity or the inconvenience of the mode of treatment may have influenced its apparent results Whenever a new of plan of treatment has failed, or has been attended with much inconvenience, it is not likely that the patient will again have recourse to it This remark may apply with peculiar force in the present case, where the plan adopted must necessarily have been met at first with a very general feeling of distrust * But the result with which we are chiefly struck is, that in the patients who were said to be cured of constitutional syphilis by syphilization, no case presented a recurrence of the disease This, we believe, is a more favourable result than can ever be expected from any plan of treatment

A very important point remains to be considered—namely, what length of time was occupied in the treatment of these cases Excluding the cases treated with mercury, and the cases in which the treatment was interrupted, Dr Sperino gives us 76 cases in which the supposed immunity against the effects of syphilis, acquired by repeated inoculation, might be observed Of these the cure is said to have been effected in less than a month in 1 case, in from one to three months in 4 cases, in from three to six months in 7, in from six to nine months in 8, in from nine to twelve months in 8, in from twelve to fifteen months in 21, and in from fifteen to seventeen months in 27 In this analysis we cannot but observe the relation between the gradually increasing number of cases and length of time required for their cure, and we are not surprised to find that no case should have returned after having been treated upon this plan from fifteen to seventeen months

We are informed that in this mode of treatment the ulcers first produced by inoculation cause some suffering, but that subsequently the patient may attend to his usual business, and appear to be in his accustomed health The cicatrices produced by the inoculations are said to be, in the majority of instances, small and superficial, leaving very little evidence of their existence Occasionally, however, the ulcerations have become phagedænic or gangrenous, and then the traces of their existence have subsequently been visible enough

The practice of syphilization is advocated for primary ulcers, which, it is said, soon lose their characteristic hardness, and begin to cicatrize Buboes also are beneficially acted upon by this treatment Vegetations, on the contrary, are not influenced by the practice But cutaneous syphilitic eruptions, secondary syphilitic ulcerations of the skin or of the mucous membranes, the loss of hair, and the deep subcutaneous ulcerations of the cellular tissue, are all said to be cured by this plan Syphilitic iritis has also been treated in this way successfully Periostitis necrosis and caries are likewise supposed to have been brought under its magic spell But before we can draw any inference from the facts stated, supposing them to be all strictly true, we should require an equal number of similar cases in which no treatment at all was pursued, and we are not prepared to say in

---

* A gentleman of our acquaintance travelling for amusement in the East thought he would improve his opportunity, and bring to bear the smattering of medicine which he had picked up in England and in some places he appears to have gained a considerable reputation among the natives He had heard that a strong solution of caustic was a good remedy for inflammation of the eyes and accordingly he adopted this plan of treatment. The result was, that he cured them all; they never came again '

which of the two classes of cases the disease would prove the most severe, [illegible] would last the longest.

In leaving the work of Dr Sperino, we have only further to remark, that he is a believer in the possibility of the transmission of secondary affections, under certain circumstances, by inoculation. He cites the case of a patient, nineteen years of age, who was admitted under his care on the 21st of January, 1851. This woman had an indurated ulceration at the orifice of the vagina, and a pustular syphilitic eruption on the back and elsewhere. On the 30th of January some pus from one of the ecthymatous spots was inoculated. Six days afterwards a little pustule formed in each of the inoculated spots, these became covered with crusts. On the 10th of February a fresh inoculation was performed with the matter taken from a pustule which had formed of its own accord upon the stomach. On the 17th two fresh pustules had resulted from this inoculation. On the same day some matter was taken from the last-mentioned pustules, and inoculated in two places on each side. Four days after this, two little points were visible on the right side, and one on the left. These formed themselves into pustules, which followed the course of those which had preceded them.

A subject affecting so deeply the sanatory condition of a large class of persons as syphilization, could not long remain without further investigation. M. Auzias Turenne, the reputed author of this plan of treatment, had, in February, 1852, requested permission of M. Pietri, the Préfet of Police, to be allowed to try this system upon the patients in the prison of St. Lazarre. M. Pietri appointed a commission to inquire into the propriety of the proceeding, and we have now before us the report of that commission. Before answering the question proposed to them, they undertook an investigation of the whole subject, and their report is divided into four parts. The first gives an outline of the objects of the commission, and their relation to the inquiries of M. Auzias Turenne. The second furnishes an account of knowledge obtained by the commission on the subject of syphilization in man, derived from the practice of M. Auzias. The third is occupied with an account of an experiment of syphilization made on a monkey, by M. Auzias, in the presence of the commission, and the fourth is the answer to the question proposed to them by the Prefet of Police, as to the propriety of sanctioning M. Auzias' experiments and practice in the St. Lazarre prison.

The commission—composed of Melier, President of the Academy of Medicine, Ricord, Denis, Conneau, First Physician to the Emperor, and Marchal, formerly Professor at Val-de-Grace—entered upon their investigation on the 12th of July, 1852. From this period they met, on an average, once a week, until the termination of their inquiry.

The first part of their report is occupied chiefly with the consideration of points to which we have already referred, and to which we shall not, therefore, again recur.

The second part of the report is that in which we expected to find a detailed and accurate account of the evidence upon which syphilization is sought to be established, but in this we have been disappointed. The amount of information furnished is of the most meagre and unsatisfactory nature.

M Auzias requested from the commission some assistance in procuring patients upon which he might try his plan of treatment, or, at least, that the commission would give its sanction and approval that he might be furnished with a certain number of beds in one of the hospitals The commission, on the other hand, declined to say that such experiments ought to be tried, and threw the whole responsibility of supplying the proofs upon which his system was based, upon M Auzias This they felt the more justified in doing, as M Auzias had described his system, in his letter to the *Prefet de Police*, as "approved"

The amount of evidence furnished in the report rests upon seven cases The first is the case of M ——, who describes himself as having become quite a new man under M Auzias' treatment "I had," he says, "during the first days of syphilization, an extraordinary appetite, and at night I had a repose which I had not known for years I have no longer any pains, and am full of energy" (p 39)

It was desired that this patient should again present himself before the commission, but he never appeared, M Auzias declaring that since his examination before the commission he had been "ebranlé" Upon an attentive perusal of this case, we must express our very serious doubts whether it was one of syphilis at all

The second case is one of great importance, and if it stood alone, would make us at least pause before M Auzias' plan could be seriously entertained It is that of a student named J——, who was attacked with erysipelas, and died during the progress of the syphilization The evidence is somewhat of a conflicting nature as to the cause of the erysipelas, and also with regard to the effects of the syphilization upon his general health M Mialet (a medical student) states that at the time the erysipelas appeared there were on the left arm eight or ten chancres covered with crusts, and others in the process of cicatrization The arm was swollen and painful

M Auzias, on the other hand, declares that the syphilizing chancres were completely cicatrized in this young man when the erysipelas developed itself

M Roby (a medical student) says that M J—— was feeble, pale, anæmic, and feverish—M Auzias, that his health was as good as need be wished (pp 50, 51) M Auzias adds, that erysipelas was epidemic in the place where this young man lived, and that a woman on the same square was also attacked at the same time, and conveyed to the Hopital de la Pitié

M Guilbert (another medical student) informs the commission that M J—— appeared satisfied with his treatment up to the last But from another source we learn, that a fortnight before his death he said, the treatment "fatigues and worries me, I do not mend The inoculations always succeed When will there be an end of this? M Auzias must tell me I will go and ask him I am always hungry, I eat like a wolf, and yet I am without strength"

It must be added that this patient had undoubtedly injured himself by excessive work and late hours, that M Auzias considered his constitution much shaken when he undertook his case, and that at one period he certainly appears to have improved in general health while under treatment

In the third instance the treatment was adopted for an ulcerated throat, and an enlarged gland in the neck, following a chancre M Auzias first made one puncture from the pus derived from the inoculated pustules of the last-mentioned case A large pustule succeeded, and was followed by a chancre as large as a three franc piece The cicatrix which was left was the size of a franc About three weeks after this first inoculation, some pus was taken from the ulcer which followed, and five punctures were made on the arm, and five pustules resulted After this, the pus derived from this patient could no longer be inoculated Some fresh pus was then taken from the last patient, and from other persons These fresh inoculations always succeeded After a time, however, some inoculations were again made, which produced no results M Auzias then having inoculated the former patient with some fresh pus, which had produced its specific effects, made fifteen inoculations with the pus resulting from this fresh source, and fifteen large pustules followed

"I suffered much" says this patient 'I had the arm swollen, and much fever, and was obliged to keep my bed for several days In scratching myself I caused a place to come much larger than any of the rest The punctures still succeeded but the last ones in a less degree I had fifty two on each arm I commenced this treatment on the 20th of February, and discontinued it on the 3rd of July, in consequence of the death of the last patient The summer before the syphilization I had some spots During the syphilization I had a very peculiar disease of the skin It appeared in red circles, with the central skin of its natural colour I showed them to M Auzias who told me that it was not a syphilitic affection My throat became quite well during the time that M Auzias was syphilizing me I slept well I had no pain, I did not lose my hair I ate enormously, without being able to satisfy myself, since then, however, I have become very thin, and lost my strength I am now nothing in comparison of what I was I am soon out of breath I had a syphilitic eruption on the palms of my hands, and my sore throat returned" (p 52)

The cicatrices on this patient's right arm were red and very apparent, without induration On the left arm they were much less visible, and some seemed to be disappearing altogether On examining the throat, a mucous tubercle was seen upon the left tonsil

M Auzias maintains that the above account is exaggerated, and that if the patient had suffered, it was from privation and want, and not from the disease

It is but fair to M Auzias to state, that when the patient made the above statement, she had placed herself under the care of M Ricord, and that the subsequent history shows that her health had undergone no permanent deterioration

The evidence furnished by the remaining cases is not more conclusive than that to which we have above referred, and as we naturally suppose that M Auzias would bring forward his most successful results, we cannot but see that he has entirely failed to prove his point

The third part of the report is occupied by the inquiry as to the possibility of transmitting syphilis to animals, but as this adds nothing to the evidence in favour of syphilization, we pass on to the conclusion at which the commission, after much pains and deliberation, arrived

There appear to have been some shades of difference in the opinions entertained by the five members, but they arrived at the unanimous conclusion, that, from the facts which had come to their knowledge, M Auzias *ought not to be allowed to try experiments with syphilization in any public institution whatever* The declaration of M Marchal (whom we have before seen taking part with M Ricord's adversaries in favour of syphilization) is remarkable

> "I became a member of the commission," he says "convinced that M Auzias was well supplied with facts which he would clearly prove, convinced that the commission would find in those facts sufficient to justify it in recommending a public trial of his experiments, and hoping that from these experiments made with judgment and followed up with diligence, some truths might result which would be honourable to science and beneficial to the human race My expectations have not been realized The facts have not been produced either in number or kind, such as I might have wished This to me has proved the greater disappointment, inasmuch as I was to a certain extent committed in favour of syphilization I believe from the facts that I have myself seen, that the following questions are still open to investigation by experiment—viz, 1st Whether inoculations, in small numbers and at different intervals, have not the tendency to cause the induration of the infecting chancre to disappear and to neutralize the syphilitic diathesis 2nd Whether similar inoculations are not capable of exercising a curative influence upon secondary and tertiary symptoms 3rd Whether they have not a marked effect upon phagedænic ulcerations, which at present often resist every known method of treatment
>
> "With regard to M Auzias, I see with sincere pain that he has not justified before the commission the demand that he had addressed to the authorities" (pp 88, 89)

We shall now dismiss the subject of syphilization and in a succeeding number we shall refer to the important topic of the absorption of the syphilitic poison

Henry Lee

(To be continued)

Review IX

Practical Observations on Diseases of the Lungs and Heart By Archibald Billing, M D, F R S, &c, &c —*London*, 1852 8vo, p 138

The duties of the critic and analyst of medical literature, involving, as they do in all cases, some of the highest trusts which can be reposed in the servant of science, become, if possible, still more enhanced when he is called on to pronounce judgment upon works ushered into the literary field with all the circumstance and influence of a name already honourably known in the annals of science If, in the case of a new and untried author, who advances bold and original speculations, it requires no small amount of moral courage to lend him support, and encourage him by our commendations should his views stand the test of a close and rigid criticism, it demands a still higher sense of duty, and an even greater measure of self-reliance and fortitude, to pronounce an unfavourable verdict on the literary labours of one whose former works we, in common with others, have respected and admired It appears to us, however, that by taking a certain standard of comparison, plain and intelligible to all inquirers, wherewith any given work shall be compared, not only is the course of the critic thus rendered free from all charge of invidiousness, but, moreover, each reader can himself apply the rule, and form his own estimate of the sentence pronounced. In the cause of science the office of the critic is sacred, and he who lightly betrays the trust reposed in him by a hasty or inconsiderate expression of one sentence of unmerited condemnation, is equally base and guilty as he who through fear or partiality withholds censure where censure is due.

In surveying the immense mass of literature which now teems almost daily from the medical press, it is well to bear in mind some of the simple primary canons of criticism, which seem to be lost sight of wholly by some authors, and very largely, if not entirely, by readers in general. The publication of a book on medicine should accomplish one or more of the following objects: The recording and addition to science of substantial facts and observations, the promulgation of original views based on observation or experiment, or, lastly, the exposition of the actual state of knowledge in a particular department of medicine, under which head, of course, may be conveniently arranged systematic treatises, and text books which, though a less high and ambitious form of medical publication, yet, when well written and composed, answer certain important and valuable purposes, educational and otherwise. These, and these alone, are the ends for which books on medicine have a right to claim admission into the literary field; these, we say advisedly, are the only legitimate ends; but we regret to say that books are written, or rather *made*, to serve other ends and objects.

If we take the above propositions as the basis of our critical observations, and in the case of any particular work, assume as a standard of comparison the actual state of science in the department of which it treats, we shall have at once the simplest, truest, and most impartial grounds for forming our judgment of an author's labours. If his work be original, we shall at once see what and how much he has added to our knowledge in any given branch of medical science; if systematic, how far his treatise embraces all that has been accomplished by previous investigators. Judging the present work of Dr Billing by the foregoing rules, we are forced, reluctantly, to aver, that in our opinion it in no way fulfils the requirements of a scientific medical publication, and we are yet quite at a loss to understand how a writer of his position, and one already so well recognised, could have been induced to commit so frail a bark to the waters. Dr Billing is well known as the author of the 'Principles of Medicine'—a work which, if it did not display great originality, yet reflected much credit on the author's erudition, research, and philosophic spirit. In the present work, however, he is neither the exponent of the existing condition of auscultatory diagnosis, nor the promulgator of new or original views on any department of this now very extensive subject. In the brief compass of 138 pages, with an exceedingly diffuse manner, and no economy of space as regards type, he has touched upon most, if not all, of the now recognised forms of disease of the heart and lungs, embracing their pathology, diagnosis, and treatment; but indeed the space actually devoted to these important topics is considerably less than what has been just stated, for we find, on referring to the commencement of the work, that thirty-two entire pages are occupied with discussing the sounds of respiration and circulation, and various theories connected therewith. Considering, then, the extremely curt and superficial manner in which these several subjects are dismissed, we cannot form any idea as to what class of readers (and every author hopes to be read) Dr Billing's work was designed for. To the student commencing the practice of auscultation, it will be all but useless, for there is no single subject sufficiently worked out to enable him to seize even its salient features, while to the practitioner who has studied physical diagnosis and thoracic pathology from other sources each page presents only a series of memoranda of what he has elsewhere seen or observed. But, independently of deficiencies of this kind, which are entirely inseparable from a work of such limited dimensions, which attempts to deal with so comprehensive a subject as that of thoracic pathology, every section of which might easily be accorded as much, if not more, type and paper than this whole book contains, there are other defects and errors, both doctrinal and circumstantial, which must be noticed.

In about six pages one of which is unnecessarily occupied with a figure of a bad stethoscope, Dr Billing dismisses the subject of auscultation of the air-passages in health, decrying all lengthened description of the natural sounds as unnecessary. Percussion is treated of in sixteen lines. Now we think no idea could be more dangerous to the student than this of the facility and readiness with which correct conceptions may be acquired of the normal phenomena. If there be one thing more difficult than another in the study of the phenomena of animal life, it is, in our mind, the elimination of *the normal constant* (if we may employ the term) from the various sources of error presented by the differences of time and intensity, individual peculiarities, the complicated interferences of age, sex, constitution, and occupation, and numerous other disturbing forces which it is unnecessary to specify. So difficult, indeed, is the recognition of this normal constant of physiological phenomena, that we regard it as perhaps the last, instead of the first, accomplishment of the observer. On this topic in particular, therefore, at the outset, we must caution the student and junior practitioner, and recommend them not to despise full and elaborated details on the natural auscultatory phenomena of the voice, the respiratory murmur, and the heart's sounds—subjects which cannot be studied with too much attention.

Dr Billing's views on the causation of the cardiac sounds have been long familiar to all acquainted with the history of auscultation. At a very early period (1832), he advocated the opinion that the sudden tension of the auriculo ventricular valves was the essential cause of the first cardiac sound, and has on repeated occasions since urged this theory, to the entire exclusion of all other sources of sound which may be proved to have a share in producing the phenomenon. Peculiar theories involuntarily lead to the observation of facts in a peculiar manner, to this alone can we attribute the author's statement with which he commences his section on "Sounds of the Heart," in which he says, "The natural sounds of the heart are nearly similar to each other"—an observation which we will only remark is fundamentally at variance with our own results, and those of others on whose faculties of perception we place the greatest confidence. It is no part of our intention here to enter into any discussion of the views now or formerly advanced to explain the cause of the heart's sounds. We will only say that Dr Billing has not only failed entirely, in our minds, to establish, as a logical conclusion, that certain causes capable of originating sound, and acting synchronously with the first cardiac sound, and in the immediate vicinity of its centre of intensity, have no share in its production; but, moreover, he has not established that the auriculo-ventricular tension sound and the normal first cardiac sound, are one and the same, identical in duration, acoustic character, and intensity. He states (p. 28), "Having, from facts established since the time of Haller, deduced my theory by logical argument *à priori*, and upon Newtonian principles, I have never sought for experimental confirmation." Dr. Billing has, we maintain, *no* logical ground for ignoring the existence of certain other alleged causes of the first cardiac sound; and yet, having never himself been in a position to judge of the precise acoustic character of the sound produced by the cause he assigns (tension of auriculo-ventricular valves), he assumes this, to the exclusion of all others, as the only cause of the phenomenon in question. Even on his own strict Newtonian principles, it would be necessary for him to establish, by direct observation, that his assigned cause was sufficient and adequate to produce the sound, which we hold it is not. In support of his views, he appeals to experiments made by two persons independently of any co-operation with himself. The first experiment is that of Mr. Harris Brakyn,* who, by means of a very rude apparatus, consisting of bladders in connexion with a flexible tube inserted into, and flanged on, the left ventricle, thought that he produced a sound similar to the first cardiac sound. As we had ourselves an opportunity of witnessing Mr. Brakyn's experiments, we feel called upon to state, that in our opinion, as far as the heart's action is concerned, they proved nothing at all; that a membranous medium receiving the impulse of a column of air or fluid, and allowed to become tense in air, will emit sound, required no demonstration whatever; but it is needless to say that this case has no analogy with the condition of things in the living heart; it is only necessary further to say, that to our ears the sound produced by the valvular-flap in Mr. Brakyn's experiment was in no way like the first normal cardiac sound. It is unnecessary to go into Mr. Halford's experiment; it likewise proves nothing. The vena cava and pulmonary veins in dogs and donkeys rendered insensible, were compressed between the fingers; and it is needless to say, that the condition of things thus produced is almost as far from the natural state as that in the case just cited.

Basing his principles of diagnosis in disease on the exclusive valvular theory of both normal cardiac sounds, Dr. Billing asserts for this department of differential auscultation a certainty, precision, and accuracy, which we think in no way accord with the experience of other observers who have given great attention to this subject, and which entirely surpass the amount of skill we have ourselves ever succeeded in acquiring. Many instances are given throughout the work, on which, as the data stated appear to us quite inconclusive, we should decidedly withhold judgment; a few occur, in which good reasons, as it appears to us, exist for coming to conclusions different from those of the author.

With regard to the present actual condition of physical diagnosis, notwithstanding the immense advances that have been made, we are not disposed to agree with many late writers, who constitute what we may term a *positive* school of observers, paying entire, if not exclusive, attention to the purely mechanical causation of vital phenomena, and largely neglecting, if not altogether ignoring, the physiologico-pathological elements of diagnosis, which we think can never be with safety omitted from our consideration of any given case. It is hard to say with which of these schools Dr Billing is to be ranked, in some places he decries all minutiæ of diagnosis and pathology, while in others he dwells with care on, and calls particular attention to, the very smallest shades of difference in the characters of the cardiac sounds, the pulse, and the mechanism by which these differences are produced. In offering an opinion on this class of questions, we do so from a conviction that errors have been and are daily committed in each of these schools of clinical study, not so much by over attention to its own doctrines and practice, as by neglect more or less considerable of the opinions and modes of observation of the other, and we are convinced that the true method of advancing diagnosis to the highest possible degree to which we may venture to hope it will one day reach, is the judicious combination of the principles and practice of the positive and the physiologico pathological schools, every possible agency of both being brought into requisition, if it promises to throw only one ray of light on any obscure question in debate.

---

* See Lancet, Nov. 24th, 1849.

We have, in the above inquiry, tried the work of Dr Billing by the standard of the actual state of the department of science of which it treats, and in pronouncing our judgment that he has not adequately represented the actual status of that department of medical knowledge, we have only to add, that we have not come to this opinion without calm and mature deliberation, and in rendering an unfavourable verdict, we have done so conscientiously, and through a feeling of what we owe to the cause of science. When he again assumes the task of authorship, we hope to meet Dr Billing on ground more worthy the name and position he has already so honourably acquired.

Robert D Lyons

REVIEW X

Handbuch der Diatetik Von JAC MOLESCHOTT, M D —*Darmstadt*, 1850
Manual of Dietetics By Dr MOLESCHOTT

THERE is annually at Oxford a sermon preached in the vulgar tongue, with the special design of warning those who a few days before have received the higher degrees of the University, against pride in human learning. Few of those present can forget the advantage taken of this anniversary—technically called "Humility Sunday"—by Bishop Wilberforce, when the British Association happened at that time to be holding their meeting on the banks of the Isis. If the hopes expressed in our last number are to be realized, and Oxford become a *bonâ fide* school of physical science, such sermons will be annually more and more anticipated by conviction, and therefore be less and less necessary. If educated persons in general were to be instructed in physiology, and knew the amount of untiring energy, strong will, and zealous hope, which have been expended by minds of large calibre in the endeavour to push scientific research to an union with practical dietetics, if they saw the incapacity for unravelling what we know must be the simplest knot, the daily overthrow of hitherto certain facts in the process of digestion, and the inability to put others in their stead, there would be no danger of their being "puffed up" by such knowledge.

The weakness of the human intellect is more strikingly shown in this branch of learning than any other, because from the earliest times it has attracted the attention of all investigators of nature and appliers of science, and the further we go back the more positive is the profession of knowledge on points of which ignorance is now confessed. So convinced were the chymiatric school of Paracelsus of the value of their "fermentation theory," that they carried it from the physiological process of digestion into the explanation of the whole circuit of life, normal and morbid. We have been witnesses of a somewhat more modest attempt in our own day. Though romance has ceased, and zeal cooled, we still see many lives of animals, and many life-times of men, given almost wholly to this subject—and with what result? That which to-day seems established by the most irrefragable experiment is to-morrow rendered doubtful by equally clear observations. Penelope's fingers are completely outdone. Well indeed may Dr. Lehmann remark, that such experience should more than any other admonish us to be reserved in our judgment upon those results of researches which appear even absolutely certain.* Might not the existence of lactic acid in the gastric juice have been so characterized? Yet has Professor Schmidt "shown" (as chemists say) its absence under many circumstances, and the presence of free muriatic, while we see M. Blondlot coming back again to his former belief in the acid phosphate of lime, though the incorrectness of this has been asserted by the best chemists of the day. Who could have expected, after M. Bernard's recent experiments on the influence of the vagus over digestion, that this influence was to be denied, or at least rendered doubtful? Frenchmen see fat resolved into fatty acids and glycerine by the contact of the pancreatic juice, while Germans can scarcely make out that an emulsion of the two substances takes place. Candidly now, is there among the chaos of different opinions offered by observers, any final cause by which to explain the action and intention of the pouring of bile into the intestines? Who could have foreseen, from the state of our present knowledge, that an isolated coil of intestine, with a little alkali inside it, would be in a condition to digest muscle? In short, the intestinal canal exhibits itself to us as the theatre of a host of most mysterious performances, yet still—

> Before the gate
> Our spirits stand disconsolate.

These considerations may fairly prevent us from wondering at the very little advance which the science of Dietetics proper has made, and the little advantage which has accrued to it from the growth of other sciences. A few of the results of old experience have been confirmed, a few rendered doubtful, empirical observation has been sometimes put in the right path, but, in truth, a conscientious writer on this subject is reduced to make a most meagre affair of the real practical part of it. And even then a greater portion of his pages is taken up with pointing out what modern science *might* ascertain, than with what it has actually discovered. Stout books, it is true, are written on the subject, but the smallest part of them is that which is named in the title-page. Take the last hand-book, which Dr.

---

* Physiolog. Chemie, vol. iii. p. 261.

Lehmann says is indubitably the best that has been written—that whose title stands at the head of this article—Moleschott's 'Handbuch der Diatetik.' It contains 600 pages of letter-press, of these 167 are devoted to general physiology, 337 to the description of animals and plants eaten by man, and 96 to the physiological action of food and the choice of diet. Valuable all this matter is, no doubt, and the more so from having the name and sanction of Professor Tiedemann attached to it as having furnished the greater part of the materials of the first 504 pages to his pupil, Dr Moleschott, but yet it is not dietetics, any more than a treatise on materia medica is part of the practice of physic. The remarks in the present article will therefore be confined to the latter 96 pages of the volume, which appear very fairly to represent the present state of science referrible to the subject.

The end of taking food being to convert it into blood, the natural division of that part of the work which relates to the "physiological action of food" is according to the *time* it takes to be so converted, the *quantity* of blood which it can make, and the *quality* or specific nature of the blood made. Hence we have the three chapters, "On the digestibility of food," "On the nutritive power of food,' and "On the special action of food on particular organs."

On the first head, the classical authors may be considered to be M Gosse, of Geneva, and Dr Beaumont. But both these experimenters seem to have viewed solubility as the *whole* instead of merely as a *contingent part* of digestibility. This of itself would vitiate their reasoning, but besides that, no distinction is made between mechanical division and chemical solution. Thus when Dr Beaumont says that beef suet was "dissolved' in St Martin's stomach in five and a half hours, he must of course mean broken up into an emulsion and the same with many of his experiments on compound aliments. Then the quantity of the food taken at once is often omitted to be noticed, and the amount of water mixed with it, so that, in fine, all we gain from their experiments is a rough idea of the solubility of several alimentary matters in a mixture of saliva and gastric juice.

In the defect of empirical information on the true digestibility of different substances in the body, Dr Moleschott derives the following general rules from chemical reasoning:

> "As all the secretions concerned in digestion contain a considerable proportion of water *the simple alimentary principles must, as a general rule, be the more digestible, the more soluble they are in water.* The chlorides and alkaline salts pass easier into blood than earths, the organic acids and dextrine or sugar, easier than starch, cellulose or fat, soluble albumen or legumin easier than coagulated vegetable albumen or fibrin.
>
> "With solution is often joined a modification of the dissolved substance, as when starch, cellulose, or pectin, are converted into sugar. And since this modification brings about the special conversion of the aliments into the constituents of the blood which indeed is to be looked upon as the final goal of digestion, it may be laid down as a further law, that *alimentary principles are digestible in the ratio of the nearness of the relation in which they stand to the last link of the series of allied forms.* In the class of starchy substances sugar is the most digestible, next to sugar comes inulin, dextrin is more digestible than gum, these two more digestible than starch, starch again more digestible than pectin or cellulose."

These laws, of course, as our author remarks, modify one another, so that substances which according to one of them stand low in the scale, in the other take a high rank. For example, our knowledge that starchy substances are convertible into fat, would make us view the latter as the most digestible of the two, till we recollect the difficulty of its solution in the digestive juices. He quotes as an instance of their being both in action together, the indigestibility of vegetable albumen as compared with animal, as being lower in the scale of assimilation, and insoluble too.

The importance of such laws as these, could they be established, is very obvious. In the choice and preparation of food they would be invaluable. But unfortunately, when they come to be tested by experiment, they are found wanting. Gum, for instance, which stands so much higher in the Moleschott scale of digestibility than starch, lies under the imputation of not being capable of absorption at all, or of being converted in any way by the abdominal fluids into a nutritive substance. Dr. Frerichs and Blondlot found that forty-eight hours' exposure of it to the action of gastric juice made no impression at all. Dr. Lehmann obtained similar results after three or four days. MM. Tiedemann, Gmelin, and Boussingault, found in the fæces of a goose and a duck the same amount of gum as had been taken in food, &c. (See Dr. Lehmann's Phys. Chemie, vol. iii. page 286.) This one instance suffices to show the danger of transferring to practical matters provisional rules, based on the imperfect knowledge of the progressive sciences. Not that such rules should be proscribed; they are most useful in giving expression to the stage of science we have arrived at, and serve themselves as tests of its truth; but they must be held light in hand, and let slip the instant that a squall of new empirical knowledge warns us to change our tack.

While inquiring into the various actions of the *separate* secretions upon aliments, it must not be forgotten that the chief point which the dietician wants to know is their *combined* operation. The required sacrifice of animal life would make these experiments expensive and tedious; but the necessity for vivisection is not apparent, as Dr. Moleschott seems to imagine, or one would be cautious in indulging a hope of seeing them carried out. Observations on the fæces, which to the microscopic eye present nothing repulsive, would probably teach as much on this head, and comparative anatomy offers its interesting and promising help. These are both preferable to the author's proposal of dissecting live creatures on a large scale.

The influence of *cookery* on food is a subject of great importance to the dietician, whether he looks on it from a purely philanthropic and economical, or from a curative, point of view. In experimenting on the *chemical* changes effected by its various operations, especially that of heat, the *mechanical* condition of the substance to be cooked must not be forgotten, or we shall find the experience of the *artiste* more valuable than that of the philosopher. A striking example of this mechanical influence is afforded by the truly British operation of roasting a joint. The radiated heat glows out of the open range from the good Wallsend (none of your Yorkshire or Midland coals for our kitchen, they get covered with dust, and do not radiate); soon the outer layer of albumen becomes coagulated, and thus the exit of that which is still fluid is prevented, and it becomes soli dified very slowly, if at all. The cellulose tissue, which unites the muscular fibres,* is converted by gradual heat into gelatine, and is retained in the centre of the mass in a form ready for solution. At the same time the fibrin and albumen, according to Mulder, take on a form more highly oxidized, and more capable of solution in water. The fat also is melted out of the fat cells, and is directly combined with the alkali from the serum of the blood.† Thus the external layer of albumen forms a sort of box which keeps together the important parts of the aliment till they have undergone the desired modification by slow heat; a box, however, permeable in some degree by the oxygen of the free surrounding air, so that most of the empyreumatic oils and products of dry distillation are carried off. These are doubtless in a general way no loss to our stomachs or our palates, but one we may hope is retained in some proportion. This is acetic acid, whose presence would certainly tend to make the muscular fibre, as well as the albumen, more soluble. The case hardening of the joint may be produced in a certain degree by *rapid boiling* but the interior albumen seems by this process more hardened and less digestible, perhaps from want of the acid above named. *Stewing* and *baking* retain all the good things, but then they retain also a variety of known and unknown educts, inimical to the stomach's peace; while *slow boiling* makes, it is true, a digestible soup, but converts the muscular fibre into a mass of hard strings, which eaten or not eaten must necessarily be wasted. Roasting, then, is as scientific and wholesome, *and therefore as economical*, a process as it is a palatable one. One great advantage which roasting can boast of is, that it puts a check upon the deception, intentional or non-intentional, as the case may be, of over preparation. Heat seems to have an effect upon albumen, in some degree proportioned to the period of its application, rendering it more and more insoluble, even after any apparent change can be perceived by the palate. Thus soups and stews which are "kept hot," are wholesome enough during the first three or four hours, *may* be digested at a railway refreshment room for some hours after, but on the second or third day give the incautious eater of two-franc Palais Royal dinners an infallible diarrhœa.

Another excellent application of the mechanical process of forming the exterior of the joint into a case for the retention of the valuable fluid matter inside, is made by M. Soyer, in his 'Modern Housewife,' (p. 155.) He directs that meat intended to be eaten cold should, immediately it is cooked, be plunged into water of as low temperature as possible, so that the exterior adipose tissue should rapidly solidify, and retain the gravy in the meat by an impermeable coating of fat. As chloride of sodium and acetic acid have both of them the property of rendering albumen more soluble in the laboratory, and perhaps also the fat more digestible,‡ there is every reason to believe that they should not be viewed merely as a means of preserving meat from decay, but of rendering it more completely alimentary at the same time. A most interesting series of experiments might be made on this point, which would have at the same time a calculable commercial value.*

---

* Not however the sarcolemma which an experiment of Kolliker's seems to remove from the class of substances yielding gelatine. See Kolliker's Microscop. Anat. vol. ii. p. 250.

† G. J. Mulder on Nutrition in relation to National Disposition, quoted by Dr. Moleschott, who has translated it into German from the Dutch original.

‡ Pereira on Food, &c., p. 173.

Practical physicians would also be glad to know under what circumstances a mixture of animal and vegetable food is more or less digestible. Most healthy persons seem to thrive best under it, and even some dyspeptics, from whose meals routine practice has excluded potatoes, and peas, and pastry, for a long period, find themselves more comfortable, when in despair they begin to live like other people. Is it that vegetables stimulate the organs of digestion to greater activity and more copious secretion? Such appears to be the case with graminivorous animals as compared with carnivora.

On the whole, it appears probable that more light will be thrown on the subject of the different degrees of digestibility of aliments from a consideration of their mechanical than of their chemical forms. A series of microscopical observations on the condition of food under various circumstances of preparation and digestion, is much wanted. It would both pave the way for further chemical research, and contribute valuable knowledge to the practitioner.

The *nutritive power* of different aliments is directly in proportion to the quantity they contain of the elements of the blood, in a state capable of assimilation. The latter clause is inserted by Dr. Moleschott especially to exclude the valuation of food solely in the ratio of the quantity of nitrogen it contains. This proposal would class perfectly insoluble fibrin, coagulated vegetable albumen, &c. among nutritive substances, besides supposing that the body is increased by them, caffein, and other similar principles, of which there is no evidence, except, indeed, what is afforded by the resemblance of their chemical formulæ to that of creatinine, and some of the uninvestigated animal alkaloids. Nutritiveness, then, depends in no slight degree on digestibility, and so far our investigations on this head will receive help from a consideration of the mechanical conditions of the several component parts of the alimentary body. While looking to the more obviously required constituents of food, it must not be forgotten that blood, and that which is to be formed of blood, contains also inorganic mineral elements. Soda, and potash, and chlorides, are required, and the food must possess these, or it cannot be said to be nourishing. Now, the researches of Liebig (quoted in Lehmann's 'Phys. Chemie,' part iii. p. 91) show a very different proportion of sodium and potassium in the extracts of the flesh of different animals. A pursuit of similar researches into the comparative amount of phosphate of lime, mentioned in the same place, as being carried on by Schlossberger and Von Bibra, may lead to valuable results. The deficiency of this salt in the flesh of young animals was observed by Liebig and Berzelius, and may perhaps help to account for the inferiority of its nourishing power, which has long been empirically suspected.

Though little positive proof of the reason for the varying nutritive power of the flesh of diverse animals can be given, one negative fact is of considerable importance—viz., that it does not depend on the quantity of nitrogen they contain. This is clear from the analyses made by Schlossberger and Kemp, which are given in the third volume of Dr Lehmann's 'Chemistry.' From these it appears—

---

* Dr. Garrod's observation that ordinary salt provisions are deficient in the proper amount of potash required by the blood, is suggestive of an improvement in the preparation of meat.

> 'That the amount of nitrogen in muscular fibre is throughout the animal kingdom essentially the same. The flesh of fish contains the same total quantity of this important element of nutrition as that of the higher animals; oysters, on the other hand, instead of containing more, as common experience would lead us to conjecture, actually stand lower down in the scale of proportion. So there is a striking difference between being rich in alimentary principles and being good for food."

Besides the necessity for being in a condition capable of absorption, it is probable, also, that there is required in the nutritive principles a certain combination among themselves in order to support life, and that on the approach to perfection in this combination the value of different aliments consists. The diet, as a whole, must contain all the constituents of the body, and we may conjecture, *a priori*, that such diet will be most suitable which contains these constituents as nearly as possible in the proportions and combinations exhibited by the body to be nourished.

Dr Moleschott is known abroad for the spirited foray on which he has spurred against those who have accepted the theses nailed up before the world by Baron Liebig, in his 'Chemical Letters,' especially those which divide food into "elements of nutrition" and "supporters of respiration." In a recent work, called 'Answers to Liebig's Chemical Letters,'* his energy almost excites a smile. He does not hesitate to pronounce "any extension of pardon, any delay in passing sentence on such a doctrine, an actual sin." For the sentence which he does pass we must refer the reader to the answer to the ninth letter. It is rather too much in the style of "the counsel for the plaintiff," and really argues rather against the Baron's incautious mode of wording his ideas, his fondness for "startling novelties" and bold generalizations, than against what may be supposed his actual opinions. The reckoning of hydro-carbonaceous food as merely fodder for a walking fireplace is very likely as faulty as the answerer of the letters makes it out; but there is good sense in Luther's dictum, *Auf ein grober Klotz gehört ein grober Keil* (a sturdy billet wants a sturdy wedge), and these rough, bold ways of speaking often impress a new truth on the multitude when more correct axioms would fail. On dieticians, medical, agricultural, and philosophical, it certainly has impressed the fact, that for the great outgoings of carbon there must be corresponding incomings; and under the wing of this, we may be allowed to avoid a question which ought to have an article of its own in some future number.

The proper proportions in which the elementary principles of food should be combined cannot be better estimated than by observing what they are in that typical aliment which the Creator has prepared for us before reason makes us cooks, and when the "whole duty of man" consists in growing big and strong. The proportions in milk are, according to Dr Lehmann, 10 parts of plastic material, 10 parts of fat, 20 parts of sugar, and 0.6 of salts. But then we must remember that this golden age has an end, that other functions and other duties arise, the outer world makes its demands, mental and bodily deeds have to be done, and therefore (have we not a right to say *therefore?*) a different expenditure and a different income must be calculated on. This is obvious enough, but as to what the differences are we are completely in the dark. We cannot even guess why different young animals should be supplied, as they are, with different fare—why the calf should be allowed so little sugar and so much fat and casein (unless, indeed, we count butter and cheese dairies as part of the final causes of cows)—why the "poor little foal of an oppressed race" should be treated to as much sugar as his rider, and much more fat. Why should kids and lambs enjoy the sweets which puppies are denied? (See Lehmann's 'Physiolog. Chemie,' part ii. p. 335, for the different constituents of milks.)

---

* Der Kreislauf des Lebens. Mainz. 1852.

> "As the founder," says Dr. Lehmann, "after he has assayed the ore, knows how to mix his fluxes in a proportion corresponding to its contents, and suitable to smelting it, so should it be the aim of the physiologist to calculate for a given organism under given circumstances the proportions in which the individual alimentary principles must be combined, so as to ensure a favourable result. In such an interrogation of nature, physiology seems to have the best prospect of keeping regular accounts. From these, again, general formulæ might be constructed, by which he might succeed in predicting with a high degree of mathematical precision, the effects of a specified agency on the animal frame. Many, indeed, are the vital acts to be taken account of in such a formula, and almost countless are the investigations required ere this goal can be won. But though broad be the field, and great the hindrances, yet rich are the rewards it promises to successful toil—rewards which will not only advance theoretic truth, but become deeply incorporated in everyday life. Dietetics will be grounded on a firm foundation, and perchance the thought is not a vain one that the healing art also may be thereby made capable of more accurate investigation.*

After discussing the nutritive powers of aliments, Dr. Moleschott enters upon their "specific action on certain organs." In speaking of their influence on the digestive canal, he attributes the good effects of condiments, such as mustard, pepper, onions, tea, and coffee, to the increase in the secretions of the salivary and gastric glands by means of their essential oil. To fruity wines he attributes the same benefit, still further augmented by the alcohol they contain. The admirers of "peaty" small-still whisky will be glad to read of fusel oil (whence its flavour is derived) being included in the same category. The inconveniences arising from the use of maize, lentils, peas, rye, and that "grain which in England is generally given to horses, but in Scotland supports the people" (Johnson), chesnuts, almonds, &c., are due, according to our author, to the great demand they make on the solvent juices. They contain little water, and therefore absorb much of that, while their cellulose so envelops the albuminoid constituents, that these last require a larger amount of gastric secretion to dissolve them than any other that the "dura messorum ilia" can supply. So that an inadequacy of the digestive solvents, that is to say, an inadequacy in respect of the demand, is caused.

The mechanical influence of the unabsorbed parts of eatables is disposed of in rather too summary a manner. That the seeds of strawberries and currants, the skins of small raisins and the like, should act as foreign bodies and excite the peristaltic action, is comprehensible enough. But why syrups, and honey, and oil, and fat, should act in the same way, while emulsin, dextrin, gum, and starch, whose obvious physical qualities are not very different, "clothe the mucous membrane with a defence against irri tating substances," is not so clear. The chemical changes which the former class are liable to should be taken into account. But, in truth, are the facts quite certain? Figs, dried currants, seedy summer fruits, and so on, are certainly laxatives, but are they so in consequence of their skin and small seeds? Tamarinds, bael jelly, and the insipid mashed-potato-like banana, are much more purgative without seeds, while, on the other hand, canary seed has no effect on the bowels, and pigs eat haws by the bushel without any consequence to themselves, though the haw, when sown, sprouts a year earlier for its semi-digestion. It may fairly be questioned whether indigestible matters, as such, really irritate the intestines, and whether, in some cases, as gum, for example, they do not soothe them.

---

* Lehmann's Phys. Chemie, part iii. p. 448.

The direct effect of substances, insoluble as well as soluble, on the mucous membrane of the digestive canal is one of great interest to the practical physician, and he has not received that industrious aid from science applied in this direction which he may justly claim. Experiments on the effects of astringents on mucous membranes, on the lines of distinction between these and tonics, on the methods of guarding them by an envelop of insoluble matter which is adopted in drugs and aliments presented to us by nature, would be of as much use to dietetics as pharmaceutics. The last point especially—namely the mechanical method of distributing a substance through the intestinal canal by enveloping it in cellulose, deserves great attention. We have all observed the different effects of bark and quinine, between the latter substance given in acid and in a solid form, by the mouth or by enema, and one cannot fail to attribute these variations to the different parts of the mucous membrane, and the different extent to which its action spreads. So, too, in food, the mechanical differences of modes of preparation must offer to the intestinal canal the component parts of the dish in a different order. For example, two sorts of pie-crust are familiarly known as "short" and "puff," in the former, the butter is thoroughly incorporated with the dough, so as to divide the starch-granules one from the other, and permeate the gluten like a sponge, while in puff pastry, the dough forms thin but solid layers, like a quire of buttered paper. If the teeth are imperfect, or the mastication careless, the latter is well known to form a solid mass in the stomach, which is very difficult of solution in the upper portion of the intestines, while the easily-broken paste is mixed with the rest of the food, and though formed of the same chemical constituents as its indigestible brother, receives a very different character from its employer.

The changes effected in the blood by a large supply of albuminous and fatty food have not been made the subject of inquiry, the general semi-metaphorical expression, that it is made "richer" by them, is not satisfactory. The author's statement that it is "thinned" by chlorides and neutral salts, is still more vague, for though, truly enough, they would render its albumen more fluid, yet they must also make the albumen of the food more soluble, and so cause the entrance of a larger quantity into the circulation. The same influence is attributed to fruit and vegetable acids, and an awful retribution of dropsies, cachexias, anemias, diarrhœas, threatened to young ladies who endeavour to reduce by vinegar those proportions that Rubens loved. It must be confessed that our author, in spite of the off-hand disrespect which he exhibits in his *l'envoi* (p. 599), for the opinions of nurses and old women, does occasionally fall into their phraseology, as here in his talk about the "richness" and "thinness" of the blood

The statement of M Lecanu, that the use of alcoholic stimulants directly augments the fatty matter in the blood, has been employed for moral and social purposes, but now some doubt is thrown upon it by Dr Lehmann, who says, he cannot find that the change takes place, except where there is a granular condition of the liver preceding* Dr Lehmann's own observations as to the increase of sugar after eating starch, and of fibrin from animal diet, are equally important, and deserve further inquiry and confirmation

The influence of diet over muscular fibre is an important social question, for thews and sinews have always ruled the world both in peace and war in a proportion quite equal to brains Indeed, it is a question, which the present writer is disposed to answer in the affirmative, whether, *nationally*, muscular and mental energy do not always run in couples, and whether the first is not the cause of the second? It does not appear that any diet, so that there be plenty of it, is incapable of fitting man to get through his daily work, but the best specimens of the species are certainly those who enjoy the greatest mixture The example of the hunting nations across the Atlantic is given by Dr Moleschott in evidence that an exclusive diet of mammalian flesh increases muscular development, but Mr Catlin and the Ioways at Lord's Cricket Ground did not bear out this statement And the inhabitant of the Pampas, who lives wholly on water and beef, made tender by being rode upon between saddle and back till dinner-time, cannot show, according to Sir F Head, an inch of calf, though use and necessity develop his arms to an unnatural extent A Brahmin sepoy, who would as soon eat his own flesh as anything besides rice, would walk him, run him, or knock him down any day, and he again would receive the same treatment from many of us, fed as our fancy leads us Feeding on fish has, according to our author, a deteriorating effect upon the size and development of the muscles, but he is not very happy in the instances he cites of ichthyophagous nations He mentions the Samoyedes, the inhabitants of the Hebrides and Faroe islands, Greenlanders, and the North-west Americans Now the Samoyedes are small enough, certainly, but do *not* live on fish; being so noted for their love of warm blooded meat, that "in the Russian chancellery they are designated *Syroyadtsi*, eaters of raw meat'"† The present inhabitants of the Hebrides would stare much at the company they find themselves in, nor were matters worse 150 years ago, for we read in Martin's 'Description of the Western Islands' (London, 1716, 2nd edition), that "the diet generally used by the natives [of Skye] consists of fresh food, for they seldom taste anything that is salted except butter Their ordinary diet is butter, cheese, milk, potatoes, coleworts, brochan, that is, oatmeal and water" (vulgo, *porridge*),—and a similar bill of fare is attributed afterwards in the same work to Tirae and St Kilda As to Faroe, their bad habits seem to have been given up now nearly a hundred years, for we read that "we have a remarkable instance of the great effects of diet on the diseases of a nation in the inhabitants of the isle of Ferro Since fishing has declined among them, and the inhabitants have cultivated corn, and live on other food instead of whale's flesh and bacon, the elephantiasis has entirely ceased among them '* As to North-west America, the public interested in the search for Sir John Franklin have been assured over and over again that an abundance of fowl, and mammalia also, sufficient for human food, is obtainable at all seasons The excessive improvidence of the natives, and the severe climate, fully account for their miserable condition On the other hand, large tribes who subsisted at one time entirely, and still in a great measure, without red-blooded meat, are noted for their corporeal development Who have so often excited the wonder of travellers for their superiority to most of the Pacific nations allied to them as the New Zealanders? Yet they are notorious fish eaters Their *carte* is deficient in mammals, dogs and swine are a recent introduction, and man, we hope, was always an occasional luxury, but a programme of their fish dinners, given us by a chaplain to their enterprising bishop, might almost draw Apicius round the globe It comprises lampreys, eels fresh and dried, kippered shark, a kind of cod, mullet, whitebait (*inanga*), which is boiled or broiled or baked in small baskets, so as to make a fish cake, cockles of three sorts, mussels, oysters, and a whole list of Maori names said to belong to dainties of the most refined description It is true they have birds and vegetables also in considerable variety, and, as before suggested, perhaps this variety causes their superiority, but still the staple of their fare is evidently fish, as observed by Captain Cook A similar mode of living is attributed by this observant seaman to the Sandwich islanders, of whom he says "the majority were above the middle height,' and to the clean, comely Otaheitans, whose frail daughters were fair enough to cause the mutiny of the *Bounty*, before European civilization had altered them So that we cannot attribute degeneracy solely to the substitution of fish for meat The truth probably is, that *the mode of procuring food has a greater influence over mind, manners and muscles, than the nature of the food itself* He that is satisfied with what he can pick up ready-grown degenerates either into a starved New Hollander, where food is deficient, or into an effeminate creature, like the former inhabitant of the West Indies, where it is abundant, he that seeks only the greatest amount of nitrogenous matter grows up a mere hunter, and becomes a prowling, cruel, passionate, dirty, yelling American Indian (for the "noble savage ' existed only in Mr Cooper's fancy), while a civilized people will be found from the earliest times, like the wise son of Sirach's man of a good heart, to "have a care for their meat and diet " They will have thought about it, laboured for it steadily, investigated nature and advanced science to improve it, and obtained their reward in the search itself

---

* Phys Chemie vol i p 259

† Quoted from the Histoire General des Voyages, xxiv, in Pinkerton's collection.

The articles endowed with most direct action on the *nervous system* are tea, coffee, and alcohol, of which Dr Moleschott compares the different effects It is not clear why he omits tobacco, which daily soothes the ruffled spirits of so many, and proves in excess so injurious to nervous energy

The operation of diet on the *generative organs*, on the secretion of *milk*, on the expired *air*, *urine*, *and skin*, are discussed by Dr Moleschott, but what has the adipose tissue done that it should not appear among those parts of the person over which food has an influence? That unhappy saying of Hunter's, that "fat is no part of the animal body," seems to be taken much more literally than he probably intended. It is true that to the fat can be assigned no one defined duty, as "motion" to the muscles, "sensation" to the nerves, "preservation of form" to the bones, but without it no one of the other parts can perform their offices, and its total disappearance is synonymous with death. Aliments which most rapidly form adipose tissue are the care of the agriculturist, and it is often of the greatest interest to the physician to know those least likely to produce it.

---

* See Von Troil's Letters on Iceland, Letter xxiv, dated 1776, from Chevalier Back to Dr Von Troil.

The succeeding chapter is on "The choice of victuals in a state of health." Modern science allows man in this exercise of volition much more liberty than till lately he claimed from his advisers. If any one desires to see the heavy bonds under which our forefathers groaned when they sought "what to eat, drink, and avoid," let him weigh Sir John Sinclair's tractate 'On Health and Longevity,' in four closely-printed octavos, 800 pages to a volume. Fancy a zealot for antediluvian life trying to act up to the insane quackeries of Lord Bacon! He would from time to time "be let blood," to "evacuate and empty the old moysture of the body," he would wear clothes of a "watery and oily substance"—to wit, greasy flannel or baize, he would oil his body daily, or paint it like an ancient Briton, and bathe in a variety of nasty expensive preparations, he would avoid change of air, because it 'wastes the body,' he would stir up the sexual affections, but not gratify them, to "strengthen the heate of the spirits," he would eat nitre with his food, dip musk roses in his malmsey, take garlic and tobacco as a duty, shun sympathy with his kind, because care, pain, and sorrow shorten the days, lose the seasoning of every pleasure by taking it as a sort of medicine instead of for the pleasure's sake, and live in such a daily state of bewilderment as to whether his food was hot, cold, moist, or dry enough—in such fear lest he ought to be drinking pearls, eating bezoar stones, or taking some similar remedy for immaterial ailments—in such anxiety about the want of hair on his legs, or other equally certain signs of long life, that living would be a task, and "Renew my age" would cease to be his prayer. But we have escaped from this bondage now, and even the rational advice which the Schola Salerni sent to Duke Robert of Normandy in rhymed hexameters, would be considered a burden. These poetical prescribers were long the guides of Europe, and though some lines have a perfectly jovial sound, such as

Ova recentia! vina rubentia! pinguia jura!
Cum simila pura naturæ sunt valitura.—

others interfere unwarrantably with personal tastes, denouncing, for instance, one's pet crusts,—

"Ne comedas crustam, choleram quia gignit adustam."—

and directing the transit of wind through the bowels, and mucus from the nostrils, in a way not to be borne by a free man.

But though the modern dietician does not venture to interfere much with the habits of his species in general, there are certain classes for whom it is incumbent that a provision should be made by others, and the arrangement of the diet of these is one of the most useful employments of science. Masses of our dependents require to be supplied with an amount of food exceeding only slightly that which is necessary to perfect health, and what this necessary amount may be calculated at is of the greatest importance to those who have to regulate the diet of soldiers, sailors, paupers, prisoners, &c. One way of ascertaining the exact quantity which will support life is the direct method of limiting the food till it is evidently insufficient. But this is not a pleasant experiment. Luigi Cornaro tried it, and by dint of living a vegetable life in his palace and gondola, protracted existence on what we should call half a breakfast a day. Dr Stark tried it, and got on pretty well so long as he had nothing to do besides taking his weight, but when he was obliged to undergo a contested election for St George's Hospital, it killed him outright—as it would have done Cornaro, St Hilarion, Simeon Stylites, or any other hero of asceticism. The lowest diet on which a definite individual can manage to exist under special circumstances is not to be taken even as a minimum for others under mixed circumstances. Professor Mulder goes another way to work, and calculates backwards from the expenditure of carbon by the lungs, nitrogen in the urea and so on, what must be the allowance of each. Dr Moleschott objects that this is faulty in principle, because it is impossible to be certain from which aliment the particular constituent of the secretion be derived. But why *should* you know? The first thing to ascertain is the quantity of each element required, and its mode of combination may be made the subject of different experiments. On such a point as this it is dangerous to venture upon numbers without going more into detail than would be consistent with the limits of a review.

Another class for whom we are bound to use our reason in providing are children, and above all those whom misfortune has deprived of the ready formed bounty of nature at an early period. The resemblance of ass's milk to human renders it the most proper substitute, though it must not be forgotten that it is more watery, and therefore that a larger supply is required. But its rarity renders it to many unattainable, and it is very desirable to prepare cow's milk in such a way as to make it suitable for the purpose. The latter contains a great deal too much cheese, a slight excess of butter and salt, and is very deficient in sugar. To reduce the excess of casein requires an equal bulk of water to be added to the milk, and the deficiency of sugar is estimated by Dr Moleschott to be made up if from thirty to forty parts in a thousand of sugar of milk are put in. He is troubled how to compensate the want of butter—would not a little cream settle the matter? Should any ass's milk be obtainable, though limited in quantity on account of the expense, a very perfect imitation of the human secretion is made with two-thirds ass's, and one-third cow's, mixed warm. In regulating the degree of dilution, it must be remembered that during the first days of lactation the milk is at its thickest, gets quickly more watery, and then gradually richer in casein and salts.

In commenting on the choice of food for different sexes and ages, the author states his opinion that the use of tea hurries forward sexual development, and therefore warns us against giving it to young girls, lest the proper time of puberty should be anticipated. He also accuses it of a tendency to cause miscarriage, grounding his statements on an experiment of Mulder's. It is to be hoped he is rash in these opinions.

In speaking of the mode of life as modifying the diet, a popular error is noticed, which supposes that intellectual labour does not exhaust the body He says that any one may find by his own experience how much the interstitial metamorphoses are hastened, and therefore a fresh supply of food demanded, by mental exertion Few deny this, but many forget it

The remarks on the influence of climate are interesting to a people who have so much connexion with foreign parts as the English A remark is made which we have heard confirmed by several of the junior practitioners from our Indian possessions—viz, that in the present day much more harm is done to European constitutions there by over-eating, than by over-drinking

The twenty pages designed to contain all that science has contributed towards guiding "the choice of diet in sickness," conclude the book The limited space shows how little is to be said on this point The road to truth seems to be that indicated by MM Andral and Gavarret—viz., the analysis of the blood Yet at the outset of our endeavours to apply to practice the few facts ascertained, we soon learn to feel the correctness of the observation "that we must not expect to be able to diminish in the blood an abnormally augmented constituent, by removing the corresponding constituent from the aliment" In fact, it is sometimes even added to, as for example, when MM Andral and Gavarret found the fibrin in a dog's blood unnaturally large, after the entire abstraction of all food So that the simplest of all direct indications becomes more complicated as we examine into it

Inflammations, fever, scurvy, drunkard's cachexia, chlorosis, diabetes, rickets, the stone, the gout, and convalescence, are the abnormal states to which sections are assigned There is nothing that would seem particularly new to the readers of this Review, indeed, they will probably wonder to see omitted much that is familiar to their minds They will miss allusion to the treatment of consumption, of which even the "*haustus-ter-die*" system has taken a dietetic turn, to the management of diseased hearts, over which muscle, more than all in the body, abstinence seems, by M Chossat's observations, to exert so much influence for good or for ill, and to the familiar experiments on aneurism Even diseases of the stomach are alluded to only when malignant, and the indications of liver-disease are not mentioned Obesity is only inserted as part of the *dyscrasia potatorum* (which it seldom is in England), and the distinction of sthenic and asthenic, so important in the treatment, is omitted The times of the day, and the frequency with which food should be taken, are learnedly commented upon as respects the healthy, with whom it is rarely a matter of choice, and never of great consequence, while the periods of the sick-bed meals receive no notice Dyspeptics would not appear to be considered as invalids at all, for they find no place in this chapter On the whole, little as is the assistance which the present state of physiology shows it to be able to give to dietetics, rare as it is to find in practice any suggestion derived from theory or just analogy, the volume reviewed really does not seem to have made the most of it, or to have set before us the whole capabilities of the subject

T C

REVIEW XI

1 *Histoire Naturelle des Vegetaux Parasites, qui Croissent sur l'Homme, et sur les Animaux Vivants* Par CHARLES ROBIN, M D, &c (Avec un Atlas de 15 planches gravées)—*Paris*, 1853 pp 702

Natural History of the Parasitic Vegetables which grow on Man and on Living Animals By CHARLES ROBIN, M D (With an Atlas)

2 *Recherches sur la Nature et le Traitement des Teignes* Par M E BAZIN, Medecin de l'Hôpital St Louis —*Paris*, 1853 pp 152

Researches into the Nature and the Treatment of Tineæ By M E BAZIN, M D, Physician to the Hospital of St Louis

THE treatise by M Robin can scarcely be considered a second edition of that formerly published under the same title Both as regards matter and manner, it is a fresh work, so widely have M Robin's researches been extended, and so much has he profited by the labour of the intervening years It contains a full account of every parasitic plant which has yet been discovered on the bodies of animals, and the descriptions are illustrated by a very beautiful atlas

The work of M Bazin is devoted entirely to a consideration of the various diseases, classed by him under the head of Tineæ All of these are considered to be dependent on the presence and growth of parasitic plants, and therefore, in our review of M Robin's work, we shall necessarily traverse the ground occupied in M Bazin's treatise

The first 252 pages of the 'Natural History of Parasitic Vegetables' are occupied with "Prolegomena' There seems to be a fascination for M Robin in what he calls the "fundamental questions of biology," and accordingly, as preparatory to a treatise on a small section of the vegetable kingdom, he favours us with a long dissertation on life in general, and on the most complex questions in physiology If such a preliminary chapter be necessary, it might, at any rate, have been only one quarter of the length, and although it gives M Robin an opportunity of stating his abstract and somewhat peculiar views on "concrete and abstract biology," we cannot but consider it provoking in no slight degree to be obliged to wade through it, before reaching the pith and marrow of the volume We will not ask our readers to follow us, but proceed at once to the succeeding portion of the volume

All the vegetables which grow on living beings are, of course, cryptogamic, and are either low forms of Algæ or Fungi Under the head of algæ, M Robin enumerates 14 genera and 38 species, under the head of fungi, there are 16 genera and 18 species Most of these plants are composed of simple cells, or of cells placed side by side, the unicellular algæ being distinguished from the unicellular fungi by containing chlorophylle or some analogous substance, and usually one or many coloured vesicles The more highly formed algæ are composed of interlaced filaments (*Trichomata*), simple or ramified, cylindrical or flattened, and containing coloured molecules, and of a reproductive system—viz, vesicles (*sporangia, conceptacles*) and spores (*sporidia*) The fungi are represented by filaments, at first simple, then ramified, and formed by a single elongated cell, or, more rarely, by several cells placed end to end (*mycelium*) The reproductive system is constituted by spores, which are seated on a receptacle, either at once or by the mediation of certain special structures, or are contained in a distinct vesicle (*sporangium*)

It would lead us too far to go into the minute anatomy of these plants, which would indeed be unintelligible without the atlas, nor shall we attempt an account of the various genera and species which are found on the lower animals We shall content ourselves with enumerating those which are found on the bodies of men

The conditions of growth of the parasitic plants on human bodies are the same as in all other cases Whenever the normal-chemical processes of nutrition are impaired, and the incessant changes between the solids and the fluids slacken, then, if the part can furnish a proper soil, the fungi will appear

The soil on which these plants grow is for the most part composed of epithelium or cuticle, acid mucus, or exudation, acidity, although favourable for their growth, is not indispensable, since some of the cryptogamia grow in an alkaline or neutral ground, as on the ulcerations of the trachea On the skin, and in the buccal and pulmonary cavities, the plants are exposed to atmospheric air, and many of the fungi absorb oxygen, and emit carbonic acid In the intestines, the nature of the gas is somewhat different, but some species grow here also Humidity and warmth are important conditions of growth, and these, of course, are always to be found in connexion with the animal body

An useful division of the subject for our purpose is afforded by the anatomical seat of the cryptogamia on the skin, or on the mucous membranes of man

A Cryptogamia on the Skin

Ten varieties have been noted in this locality We shall enumerate them in the order in which they are given by M Robin

1 *Trichophyton tonsurans* (Malmsten) — Syn *Trichomyces tonsurans, Mycoderma of the Plica polonica, fungus of the hairs in Herpes tonsurans, fungus of Porrigo scutulata, Achorion Lebertii, fungus of the Teigne tondante* (Bazin), *Rhizo phyte* (Gruby)

This fungus was discovered and described in 1844 by Gruby, in the disease called by the brothers Mahon "Teigne tondante," by Cazenave 'Herpes tonsurans,' by Erasmus Wilson "Trichoses furfuracea," (one of the diseases called ringworm and porrigo scutulata in this country) It exists also, as pointed out by Gunsburg, in the Plica polonica, although the two plants were formerly described as different

The Trichophyton is formed by oval transparent spores, which give rise to articulated filaments Its anatomical seat is in the *interior of the roots of the hairs* The hairs and fungi simultaneously increase, the former seem larger than usual, are paler in colour, lose their elasticity, soften and break off when they have risen some one or two lines above the surface of the scalp, in the short cylinder then left the fungus grows still more rapidly, so that the normal structure of the small stump of hair soon becomes indistinguishable Sometimes the hair breaks off before emerging from the skin, and the fungus, epidermis, and sebaceous matter fill the ends of the piliferous conduits, and form the little prominences which can be seen by the naked eye in this disease, and give the skin a rough, anserine appearance The sporules and mycelium of the plants can sometimes be seen, in the form of a white powder, on the roots of the broken hairs, sometimes the cutis becomes congested and thickened, and then the plant is mixed up with scales of epidermis, with fatty and albuminoid granules, with pus, &c , and crusts are formed of greater or less thickness, in which the growth of the fungus can go on

MM Robin and Bazin adopt unreservedly the opinion that the Trichophyton is the cause of the disease known under the various names above given, and each author relates examples of the contagion of the disease by transmission of the spores Bazin has made the very important observation, that the same disease will attack horses, and can be communicated from them to men Both Robin and Bazin, however, admit that there is some condition of the hairs (dependent, no doubt, on constitutional causes) which is essential for the growth of the plant, as sometimes the disease disappears—i e , the fungus dies—without treatment

The *diagnosis* of this disease is extremely easy The usually round bald patches, with the little elevations caused by the swollen roots, and the dryish scales of epidermis covering the skin more [illegible] less, and accumulating round the elevations, are very distinctive marks Occasionally, when the cutis is more congested, and the crusts are thicker from abundant cuticle and exudation, some doubt may exist, but then the Trichophyton can be usually found in the crusts

The *treatment* of this species of ringworm has been long one of the most difficult points in dermatology Its principles, however, are now well understood, and few cases resist the proper measures The essential point is to apply to the roots of the hairs a preparation which may destroy the Trichophyton, if this can be done, the disease is cured It is first of all necessary to remove the hair this is in part generally accomplished before the case comes under treatment, by the course of the disease, if it has not been sufficiently done, "epilation" can be accomplished by a chemical agent, or by extraction with pincers M Bazin recommends the ointment given below,* or the oil of cade which appears to be the best depilatory known, or with these means epilation with the pincers may be combined † The removal of the hairs permits a "parasiticide" solution to be applied to the hair-follicles, within which are the prolific spores of the fungus For this purpose M Bazin recommends either a solution of bichloride of mercury (1 part to 250 of water), or an ointment of the acetate of copper (1 part to 500 of lard) We have used also, with excellent effect, a solution of the pernitrate of mercury, about 1 part to 30 or 40 of water, this is, however a very powerful remedy, and is to be cautiously used, as it easily blisters the scalp We have used also an ointment composed of sulphate of copper (1 part), alum (3 parts), and lard (20 to 30 parts, according to the age of the patient) Probably, however, a better parasiticide agent than any of these is the sulphurous acid, which we have seen Dr Jenner employ lately in a case of favus, with astonishing results, and which doubtless would be equally successful in Tinea tondens * Chlorine water might also possibly answer the same purpose

---

* Lime and carbonate of soda of each one part lard 30 parts

† M Bazin very justly remarks that the method practised by Mr Plumbe of pulling out the hairs with the pincers has been much too severely criticised by Alibert If the hairs are pulled out in the proper direction there is very little pain, especially after the sensibility of the skin has been blunted by the use of the oil of cade

With respect to the name of the most common disease in which the Trichophyton tonsurans appear, the term used by Cazenave (Herpes tondens) is extremely unfortunate, no doubt vesicles are sometimes seen, and sometimes the cryptogamic disease succeeds to true *Herpes circinata* of the scalp, but in many cases there are no vesicles at all throughout the whole course of the disease The term used in this country, *Porrigo scutulata,* is inconvenient, as it is applied with greater justice to favus The old term of tinea is, after all, by far the best, and the specific affix tondens expresses well one great feature of the disease, the baldness arising from the brittleness of the hairs †

2 *Trichophyton* (?) *sporuloides* (Robin) — Syn *Mycoderme of the Plica polonica*

In addition to the former species, Walther describes, in the Plica polonica, oval or circular flattened sporules, which have been too little studied at present to permit their exact characters to be stated

3 *Trichophyton* (?) *ulcerum* (Robin)

Lebert has described a fungus in the crusts covering an atonic ulcer of the leg We refer to Lebert, 'Atlas, pl xxii, fig 7, for a description

4 *Microsporon Andouini* (Gruby)

This plant has been studied by Gruby, and its existence (though denied by Cazenave) has been confirmed by Robin It is present in the disease commonly called, after Willan, Porrigo decalvans, or Alopecia circumscripta, or, by Bazin, Tinea achromatosa It differs from the Trichophyton of Tinea tondens, by its numerous waved filaments, and by the extremely small size of its sporules It is not found, like the Trichophyton, in the interior of the root, but forms round each hair a little tube, the hair then becomes opaque, softens, and breaks off The alopecia is rapid, with or without previous vitiligo of the skin, the dermis is not congested, and the epidermis is thin and smooth

There is an affection which should, we think, be distinguished from the Porrigo decalvans (or Alopecia circumscripta), and which is characterized by a rapid disappearance of pigment from both skin and hair, with or without alopecia M Bazin includes it in his "Tinea achromatosa' (Teigne achromateuse), but does not mention the fact that alopecia is not constant,

---

* See Medical Times and Gazette August 1853

† Bazin has lately proposed the following arrangement of the various diseases called tinea—we add the common synonyms

1 Teigne faveuse (Tinea favosa—Porrigo favosa—Favus—Porrigo scutulata)
2 Teigne tonsurante (Tinea tondens—Herpes tondens—Porrigo scutulata trichoses)
3 Teigne mentagre ou sycosique (Mentagra—Tinea mentagra)
4 Teigne achromateuse (Porrigo decalvans—Vitiligo)
5 Teigne décalvante (Alopecia idiopathique—Porrigo decalvans)

Exception may be taken to the two last terms; the others are excellent he states that a parasitic plant is present, but does not describe it There must, however, be surely something more than a fungus to cause the total disappearance of pigment from a considerable portion of dermis Besides, when the hairs return, they are at first quite white, and only gradually regain colour, but if the vitiligo were owing to a plant, they would surely not grow at all? The disease appears to us to be allied to those obscure pigmentary changes which have a much deeper seat than the surface of the body

5 *Microsporon mentagrophyta* (Robin) Syn *Mentagrophyte* (Gruby)

This is a plant resembling the preceding, but possessing larger spores and filaments, it was discovered by Gruby in a case of mentagra, and has been since described by Bazin Its seat differs from that of the preceding, and from that of the Trichophyton, it is between the bulb of the hair, and the follicle in which the bulb is seated, and never extends beyond the surface of the skin

Mentagra is usually easily cured by epilation, and by the subsequent application, on one or two occasions, of a lotion of bichloride of mercury

6 *Microsporon furfur* (Robin)

In 1846, Eichstedt discovered a cryptogamic plant in the disease called by Willan, Pityriasis versicolor, and more lately, Chloasma Soon afterwards Sluyter* described the same fungus—and lately Sprengler† has described and figured it It forms, with the epidermic scales the yellowish brown scurf seen in Pityriasis

7 *Achorion Schœnleinii* (Remak) Syn *Oidium Schœnleinii Mycoderma of Tinea favosa Porrigo-phyte* (Gruby) *Fungus of Favus*

Schonlein was the first to suggest that the honeycomb, or yellow favous crusts in the so called Porrigo lupinosa (Willan), and Porrigo scutulata, were constituted by a vegetable growth This has been repeatedly confirmed, and many excellent descriptions have been given of the disease, now called indifferently Favus, Tinea favosa, or Porrigo scutulata, but none, we think, better than that which is contained in the work before us

M" Robin believes he has discovered that the primary seat of the Achorion is in the depth of the hair follicle, against the hair, and, as well as we can understand the description, outside the layer of epithelium which covers the root of the hair, and which forms the "inner root-sheath" of Kolliker ‡ In this observation, however, he has been anticipated by Wedl,§ who has pointed out that by using a concentrated solution of liquor potassæ, to make the parts transparent, the fungus is found in the follicle round the hair at the place where it passes through the epidermis In addition to this, the plant is found in depressions on the surface of the skin, forming the yellow honeycomb-like masses which give the specific name, favus, to the disease, and which, from their frequent buckler-like shape, suggested the term "scutulata" The development of the Achorion in this situation is described by Robin after Remak and Lebert A cuticular elevation is seen, beneath which is a small favus, when the cuticle is raised, a drop of pus sometimes issues, hence the error of those who have considered this disease always pustular, generally, however (Robin, Simon, and Hoefle), there is no pus or liquid of any kind, the plant grows, and the cuticle over it (supposing it has not been forcibly detached) finally separates, leaving the favus exposed to the air

---

* De vegetabilibus organismi animalis parasitis ac de novo Epiphyto in Pityriasi versicolore obvio Diss Inaug Berlin 1847

† Canstatt s Jahresb for 1851 Band iii p 182

‡ Dans la profondeur du follicule pileux contre le poil mais habituellement en dehors de la couche unique de cellules d epiderme qui lui donnent l aspect réticulé en travers se trouve adhérent le vegetal fait que n a pas encore eté noté (p 442)

§ Ueber Pilze in Favus-Haar Zeitschrift der k k Gesellschaft der Aertze in Wien, 1849

M Robin does not notice the important statement of Simon, that at first there is at the point where the favus is about to form, only an increased secretion of epidermis, he notices briefly the fact, that sometimes the under surface of the favus is coated by cuticle, which separates it from the compressed and attenuated derma

The structure of the favus is given at length by both authors, but it is scarcely necessary to do more than notice that Robin, in addition to the mycelium, the spores, and the receptacles of the Achorion, describes a finely granular amorphous layer, which forms the external coat of the favus, and is the representative of the amorphous "stroma" which often accompanies the mycelium of algæ and fungi In the favus, also, as we shall presently see, another and distinct fungus can sometimes be found

M Bazin describes the favus under three heads, which are fundamentally identical, and different only in respect of form

1 Favus urceolaris disseminé this corresponds to the Porrigo favosa, Favus dispersus, Teigne alveolaire, of other authors

2 Favus scutiformis this is the Porrigo scutulata, or Favus confertus

3 Favus squamosa, a form usually called scutulata, but distinguished chiefly by the irregular distribution of the Achorion, and by the furrowed masses formed by the fungus, the hairs, epidermis, and exudation

The treatment of favus recommended by Robin and Bazin is epilation, and the application of the corrosive-sublimate solution, or of acetate-of-copper ointment (1 part to 500 of lard), to kill the plant still remaining adherent to the hair follicle We suspect that the sulphurous acid employed by Dr Jenner will be found a more effectual application than either of these two

8 *Puccinia favi*

The Achorion constitutes, with epithelium and a little exudation, the mass of the favus, but it has been lately (1850) observed by Ardsten of Christiania, that a different fungus, a species of Puccinia, is occasionally also present Robin considers it to be only an epiphenomenon, and that it is certainly not present in all cases The Puccinia is easily recognised, it has one extremity (the body), rounded and composed of two cells of unequal size, a superior and an inferior, the other extremity is prolonged into a jointed stem or trunk

There are still three other plants found occasionally on the skin, which need merely be enumerated

9 *Mucor*

In senile gangrene, an ill-described fungus, supposed to be the Mucor mucedo of Linnæus, has been seen on the sloughing mass

10 *Aspergillus* (?)

In the wax in the external meatus of the ear, Mayer many years ago described a fungus, and Pacini has lately made a similar observation

11 *Leptomitus* (?) *of the Epidermis*

An alga has been seen by M Gubler in the epidermis of an arm which was irrigated for a long time to keep down inflammation after gunshot wound no one else has noticed it

Some of these plants may be held to be of little interest to the practical physician, but this is not the case with others Not only MM Robin and Bazin, but Simon, and others of the best dermatologists of Europe, have adopted the opinion that the plants are the actual causes of the diseases in which they are found The contrary opinion is perhaps generally held in this country, on the grounds that fungi generally are the proofs and consequences of decay, but not its causes, that in the various forms of tinea, a special condition of the skin and hairs appears necessary for the growth of the plant, and that in Tinea favosa (Favus), in particular, a marked feature of the disease occasionally is an hyper-secretion of epithelium, and exudation, owing to an hyperæmic cutis, before any trace of fungus can be found Nevertheless, these arguments, strong as they are, seem to be overborne by the two grand facts that Tinea tondens, and Tinea favosa, can be communicated by transfer of the plant, and that the disease can be cured with the greatest readiness by the chemical agents which are most destructive to vegetable life That a special nidus is necessary may very well be admitted by the partisans of this view, since, even in the case of epidemic agents, a predisposition is necessary, yet no one dreams of confounding the co-operating cause with the special and peculiar poison

It may be desirable to recapitulate the diseases of the skin in which parasitic plants are found

1. Tinea tondens, in which the Tricophyton tonsurans is present
2. Tinea favosa, in which are present the Achorion Schonleinii, and the Puccinia favi (in some cases)
3. Mentagra, or Tinea mentagra, which exhibits the Microsporon mentagrophyta
4. Pityriasis versicolor (Chloasma), in which the Microsporon furfur occurs
5. Porrigo decalvans (Tinea achromatosa), in which the Microsporon Audouini is found
6. Plica polonica, in which the Tricophyton tonsurans and Tricophyton sporuloides are present

B CRYPTOGAMIA ON THE MUCOUS MEMBRANE

The plants forming on mucous membranes, or in the contents of cavities lined by mucous membrane, are of less interest than those which grow on the skin, as in most cases they are decidedly only secondary We shall merely enumerate them

1. *Cryptococcus cerevisiæ* Kutzing (Syn Torula cerevisiæ—the yeast-plant), in the bladder, stomach, intestines, &c
2. *Merismopœdia ventriculi* Robin (Syn Sarcina), in the stomach, intestines, &c
3. *Leptothrix buccalis* Robin (Syn Alga of the mouth)
4. *Oscillaria* (?) *of the intestines* Farre
5. *Leptomitus urophilus*, Montagne (An alga, described as forming in the urine It has as yet been scarcely studied)
6. *Leptomitus* (?) *Hannoverii* Robin (Alga found by Hannover in the pharynx and œsophagus)
7. *Leptomitus* (?) *of the uterus*
8. *Leptomitus of the uterine mucus*
9. *Leptomitus of the eye*
10. *Oidium albicans* Robin (Syn Cryptogamia of diphtheritis and aphtha) Aphtophyte (Gruby)
11. *Fungus of the lung* Bennett
12. *Fungus in the discharge of glanders*

The algæ which so rapidly form in the unhealthy mucus, or in the exudation of the mouth, uterus, &c , which M Robin refers doubtfully to the genus Leptomitus, are too well known to require comment

The Cryptococcus cerevisiæ (the old Torula or yeast plant) is well described Robin does not consider the presence of sugar in the urine as essential

Robin describes at some length the Sarcina ventriculi of Goodsir, which has been lately considered to be a Merismopœdia, but does not add any thing new or important He shares the now most prevalent opinion, that the Sarcina is perfectly innocuous

In the description of the Oidium albicans (the alga of thrush and diphtheritis), we find nothing which can be added to the account given by Berg, Gruby, Vogel, Hannover, &c This historical account of the observations made on the plant is imperfect

Although we have touched on the part of M Robin's book which is most useful to practical men, we cannot avoid noticing the elaborate account given of the fungi which form on the lower animals, the description of the Botrytis bassiana, the fungus of the Muscardine, the silkworm disease, is, in particular, extremely interesting

In the appendix, M Robin alludes to the so-called cholera fungi, and has copied into the atlas the plates given in the 'London Journal of Medicine' He has nothing original to say on this point, but appears to agree with the opinions of Baly and Gull (or Sull, as M Robin chooses to write the name of the latter distinguished physician)

Finally, we can recommend M Robin's work, not only for the fulness and accuracy of his descriptions, but for the beauty and exactitude (as far as we can judge) of the figures in his atlas

E A Parkes

Review XII

1 *A Bill intituled 'An Act to amend an Act passed in the Ninth Year of Her Majesty, for the Regulation of the Care and Treatment of Lunatics'*

2 *A Bill intituled 'An Act to consolidate and amend the Laws for the Provision and Regulation of Lunatic Asylums for Counties and Boroughs, and for the Maintenance and Care of Pauper Lunatics, in England'*

3 *A Bill intituled 'An Act for the Regulation of Proceedings under Commissions of Lunacy, and the Consolidation and Amendment of the Acts respecting Lunatics and their Estates*

The introduction to Parliament of three new Lunacy Bills, containing many penal clauses for medical men who offend against their provisions, appears to justify one more examination into the principles and foundations of our knowledge of insanity

The collective wisdom of the nation assembled at St Stephen's makes enactments and statutes respecting the treatment of lunatics, and the disposal of their property, but attempts not to indicate the nature of that condition which shall be called lunacy This tough subject is one with which Parliament law could never grapple, it has always been left to the judges, and has therefore, like other judge made law, been developed from a remote and barbarous antiquity, until it has gradually assumed its present shape

Were the records extant, it would not be less curious than instructive to trace the development of the common law on lunacy from that distant period when the ravings of the madman were listened to as the inspired wisdom of the gods, through those more enlightened and not very remote times when madness began to be recognised as a disease, though demoniac possession was still credited, and poor old women were judicially dismissed from life for the crime of witchcraft From the decisions of Chief Justice Hale we would willingly trace the progress of opinion to our own times, but the running to and fro over this ground has been so great, that the onward track is lost, so we must come at once to the highly important authoritative and decisive expression of the law as enunciated by all the judges, in answer to the interrogatories of the Peers after the trial of McNaughten This consummation of common-law wisdom in lunacy was so perfect that it gave way on the first strain * The fragments, however, are supposed to do service at the present time

A great jurist tells us that judges make law for us as we make law for our dogs, they do not tell us what we must not do, but they permit us to do it, and then beat us, so that by observing and remembering what others are beaten for, we may contrive to escape the lash But some unfortunate dogs are incapable of this amount of inductive ratiocination, therefore they are not responsible, therefore they must not be beaten It has for some time, however, been professed that legislation ought to be guided by the supposed principles of *human* nature, and although some criminal and lunacy laws not very ancient now appear as dreadful and as inconsistent with human happiness as the surgical armamentarium of Ambrose Paré, we may be permitted to think that they also were constructed with the best intentions, according to the amount of knowledge possessed by the workmen of the period unfortunately, they cannot with equal ease be securely lodged within the glass cases of museums, for in common law every new instrument is but a filing-down or a tinkering-up of some old one Even these modifications are made in obedience to public opinion, for in a free country law is said to be the practical expression of the information and spirit of the age, though it cannot be denied that in administration it usually lags far behind its ruler and preceptor But the advancing opinion of the many is formed by the knowledge of the few, 'tis a diffused warmth and light radiated from the altars of science, and a public duty is thus imposed upon the humblest acolyte, to take care that in his particular department of the service this light should be as steady and clear as the materials at his use will permit

---

* Mr Justice Williams' Charge on trial of William Frost

Medical men are and ever have been discoverers They roam through the wide fields of science unrestrained by the ligatures of mouldy precedent and time worn prejudice, on all subjects which they investigate with the untiring labour of love, their information and opinions are necessarily in advance of the age, still more in advance, therefore, must they be of the opinions current in tribunals which tardily represent those of the public, and ever reflect more of the past than of the present Medical witnesses must therefore not expect to advance the rational administration of lunacy law, either by voluntarily airing their theories in the courts, or by permitting them to be drawn out for the amusement and profit of those hair-splitting and adroit word fencers, the barristers Theories which may bloom into the fruit of truth under the favouring shade of retired study, will inevitably wither, die, and be blown into the "outermost limbo" of nonentities, by the ungenial blasts of our courts of law We would ourselves never venture in the Courts to speak of insanity otherwise than as farmers talk of fallows and crops and bullocks, for so long as a man keeps to what he positively knows, his position, even in the witness box, is inexpugnable, but if he commences to show reasons, it is not certain that he will be able to prove that two and two make four

No theory or explanation of insanity, therefore, will have a chance of vitality in the courts unless it is introduced there under the ægis of public opinion, to gain this protection it must be consistent with the reason and feelings of mankind, simple and uniform in its construction, and not liable to part amidships under any strain to which facts or sophistry may be able to subject it Do those whose duty it is to lead opinion possess such a theory and explanation?

The only definition of insanity which will embrace all its forms appears to be this,—that it is a general term used to express that mental condition which is opposed to sanity We see no objection to comprehending in this manner the delirium of drunkenness and of fever with the more recognised forms of insanity these states are essentially forms of insanity, although differing in their cause and progress from others

Probably such a definition would not be deemed wholly satisfactory and sufficient by learned cross-examining counsel, but no other could be offered either with exactitude or safety.

We apprehend that the strict meaning of the term definition is, fixing verbally the limits or boundaries of some subject or matter beyond which it does not exist, as we may say the land is defined by the sea, or the sea is defined by the land, though in coral districts the encroaching land, and in alluvial flats the encroaching sea, may sometimes leave the boundaries indistinct. If, however, it is not a definition, but a description, which the learned gentlemen require, in order that they may swoop down upon some unfortunate defect or redundancy, we will not, in lieu of such, propound any fascinating theories, we will honestly and fully detail all the observed phenomena, only we nourish a secret hope that before commencement the court will adjourn to refresh.

But suppose the definition accepted, the fair rejoinder would be, What is this condition of mind which defines insanity? what is sanity? Against this it would be puerile to seek refuge in an alternating definition. A short description must be hazarded. The following appears to us simple and compendious; one less liable to objection could scarcely, we think, be devised, although even this affords an opening to the perplexities of necessity and responsibility. Sanity, then, is that condition of mind which enables a man to discharge his duties to himself, his neighbour, and his God. The judges place sanity and responsibility of man in his knowledge of right and wrong; we place it in his ability to *do* the right, and to *avoid* the wrong. We do not believe that knowledge is always power even over oneself. If it is not, how can it confer responsibility? But so long as we retain the power of *doing* or *avoiding*, we are truly answerable for our actions. When we lose this power, we become the puppets of circumstance; while we possess it, we are sane, moral agents; when it is gone, we are insane.

Here is the fundamental question of free agency, of voluntary action of conscience, the whole philosophy of ethics, which we must not pursue, but merely point to as the basis of mental pathology; for the laws of deviation cannot be determined until the laws of healthy action are established. Pathology does not precede physiology, although it may sometimes reflect back a ray into some dark corner of the parent science. A true theory of insanity cannot therefore precede, but must follow, and be auxiliary to, a true philosophy of the human mind; although to some extent the phenomena of insanity may serve to elucidate and to verify the deductions of philosophy, as the disturbing causes of planetary motion served to prove and to confirm the great laws to which that motion was subject.

Hence it follows, that the system of philosophy being given, its legitimate offspring in the theory of insanity may be inferred. Sensationalism will refer all cases of insanity to somatic disease; idealism will incline its disciples to suppose a perverted action of the soul; the ethical school of utilitarianism will place insanity and irresponsibility in want of knowledge of right and wrong; its opponents will place them in the want of power.

If the partial truth contained in these theories be mistaken for entire and universal truth, its pursuit, conducted with logical fidelity, will not fail to lead to conclusions, whose incorrectness and absurdity will be apparent.

The trail of either sensationalism or idealism thus followed, without lateral deviations, will conduct to those morasses of scepticism where "Strauss may shake hands with Diderot" In mental pathology, the one will lead to the repulsive doctrines of Heinroth, and the other to those of Mr Atkinson and the psychology of Miss Martineau's cow

A witty author says, that "in 1763, Bishop Berkeley destroyed this world in one volume octavo And a few years later, Mr Hume did the same for the world of mind, so that with all the desire of destruction remaining, there was nothing to exercise it upon" In a manner somewhat similar, the psychists have disallowed the influence of the brain in the production of insanity, and the somatists have repudiated the interference of the mind, so that between the two we see not what remains, except what Oken calls "the essence of nothing"

The only safe path in these difficult regions appears to be that of a discriminating eclecticism, which will permit the psychologist to recognise the instruction derived through the senses, and the light afforded by the reason, without forgetting the arbitrary power exercised over the conduct by the will and the emotions This philosophy will also leave him at liberty to recognise the full instrumentality of the cerebral organ, although it will not forbid him to entertain the belief, that the servitude of the mind to its organ may be not dissimilar to that which the mighty genius of Aladdin's lamp paid to a paltry vessel for the convenient combustion of oil and cotton.

Most curious and instructive would it be to trace the opinions which, in various ages and countries, have been held upon the nature of insanity, most of them, indeed, containing a certain leaven of truth, but spoiled, obscured, and overgrown by the heterologous formations of systematic dogmatism

It is said that the Medicean effigies of female beauty, which shows "what mind can make when Nature's self would fail," was constructed by selecting the perfect portions from imperfect forms, and joining them together in a consummate whole If a similar operation could be accomplished with the theories of insanity, a true and complete system might be expected to result Until the hour and the man have arrived for this work, it will be useful employment for more humble artists to sharpen and prepare the tools and rough-hew the block

Most medical men entertain on this subject some theory or other of a character sufficiently decided and explicit, they find such a bond necessary to connect the groups of phenomena they observe, besides, at the present day, the bias of the medical mind is set strongly against empiricism, and indefatigably strives to found its practice upon reason Here and elsewhere we use the term theory, not in its unfavourable sense of something speculative and visionary, but as a philosophical explanation of phenomena founded upon principles which have been established by independent evidence In this sense, therefore, and for these reasons medical men entertain theories of insanity In Germany, men are thus divided into *psychists-somatists* and *somato-psychists* in this country, we apprehend that the first and most philosophical of these sects has scarcely a place in the profession, the two latter sects divide us, a large proportion go still further than the somatists, and frame their notions according to the phrenological formulæ Not that any considerable proportion of the profession are professed believers in the so-called science Were this so, it would indicate that the facts upon which it is built are more evident and uniform than they are generally acknowledged to be But men who have not time or opportunity to investigate the intricacies of metaphysics, on which, notwithstanding, they feel themselves compelled to form opinions, are apt to content themselves with the most plausible, the most simple, and the most familiar explanation which presents itself, and thus the phrenological theory comes to be accepted as a royal road to mental science Sir James Mackintosh ascribes much of the long popularity of Epicurus "to that mental indolence which disposes the mind to rest in a simple system comprehended at a glance," and we apprehend that much of the popularity of Gall's system must be attributed to the same cause

It is astonishing with what tenacity a simple notion, whether it be true or false, adheres to the mind, let the staunchest materialist constantly hear that a certain place is haunted, and in vain will he endeavour to exorcise the ghost from his imagination, whenever he happens to be in that place By a similar association of ideas, the ghost of phrenology intrudes itself whenever we think of the human mind How difficult is it to realize the fact that "the great globe itself' is not surrounded by those zonular, equatorial, equinoctial and other lines, which its pigmy representatives exhibit for the use of schools, sometimes even grave writers are caught in describing their existence,* and, though no believers in the art of phrenology, we not unfrequently discover ourselves tracing out organography on the bald pate of some subject placidly unconscious of the operation

But in addition to this tinge which phrenology gives to the mind of the incredulous, a certain free and easy, but not very respectful, belief in its doctrines is sufficiently common, and in this diluted form they are accepted as the basis of opinions on the nature of insanity

Very many persons, whose observations have led them to refuse any faith in the details of the system, yet profess to believe in the great regions Thus Guislain says, "Si je regarde comme très problématiques les indications anatomiques que Gall a fournies, sur les compartments cérébraux, j'admets néanmoins avec une profonde conviction l'existence de certaines zones fonctionelles dans la masse encéphalique" (p 173) He admits, indeed, at p 165, vol iii, that "dans son application à la pathogénie mentale, le systeme phrénologique ne nous guide ni quant a ses détails, ni quant aux eléments anatomiques indiqués" The world is full of such half-and-half believers on all subjects, but phrenology cannot admit them within its pale because, if it is anything, it is a science constructed from the detail of simple facts, as language is constructed from words and signs However, these regionalists, who are numerous in the profession, admit the science far enough to accept its formula as the basis of their views on insanity

Again, the medical profession forcibly influences its members to look upon man, in all his relations and bearings, physiologically, and of systems of mental philosophy, that of Gall is the only one worthy of the name, which founds itself exclusively upon bodily organization and function add to this, that it explains the most occult phenomena with which we are

---

[*] See Miss Carpenter on Juvenile Delinquents, 'Athenæum,' March 19, 1853

cognizant with a completeness and perspicacity most attractive to those whose attention and studies have been fixed upon the objective world, and who are for the most part unfamiliar with the trains of thought suggested by the subjective realities of existence. These considerations taken together will account for the prevalence of ultra-somatic notions of insanity among the members of the profession.

If phrenology be true, it will afford a solid resting-place for the foot weary with floundering in metaphysical quagmires. But order and simplicity are not always synonymous with truthfulness and reality; the olla-podrida of the metaphysicians contains real nutriment, while the regular spread of the phrenologists may be but a Barmecede feast after all; the apparently solid and orderly arrangements of phrenology may be but a mirage, "the baseless fabric of a vision," while out of the chaos of metaphysics the divine force of genius may be destined to evoke a real world.

We have no intention to enter, in this place, upon a formal description of this subject; we have for many years carefully investigated the facts of the so-called science, as they presented themselves to us, not in the spirit of hostile criticism, but in that of a student willing and wishing to believe. The opinions we have been compelled to form may briefly thus be stated. In healthy individuals a fair-sized and symmetrical head appears to be the result of healthy cerebral growth developing itself *equally in every direction*, under the most favourable circumstances. These circumstances are also, in the great majority of instances, favourable to the most perfect development of the intellectual and moral faculties of man. In the majority of instances, therefore, a well-shaped head will be coincident with a well-developed mind; but it will require observations collected with more discrimination, and recorded with more accuracy, than any which the phrenologists can at present adduce, to show that this coincidence takes place more frequently than a similar one between the shape of the countenance, of the hand, or of the human figure, and that of the mind. Long before Gall's time, no less a person than Lord Verulam observed that crooked persons were generally even with nature, by the crookedness of their dispositions.

This view of the matter may, therefore, be applied to deviations from true symmetrical shape: thus, from hereditary tendency, or the influence of circumstances, a man's bodily frame and organization may develop themselves in the lateral mould, or that best adapted for strength and resistance; the chest may become broad and capacious, the bones thick in proportion to their length, the hands short and muscular, and the cerebral organ will also itself be developed laterally. The same circumstances which tend to produce this type of physical man, tend also to form the corresponding mental character, with vigorous appetites, great strength of will, and of the pathematic functions of the mind. Vigorous functions are not usually easily moved or highly sensitive; therefore in this type of character the intellect is often slow, it is frequently also untrained by exercise, the other portions of the character inducing this kind of individual to avoid study, and to engage in the active turmoil of life. Reverse all the conditions, and we have the highly intellectual man, with emotions quick and vivid rather than strong, with elongated hands and frame, and an elongated brain In these co-existences of certain physical types of human form with certain mental types of human character, we see ordinary examples of coincidence of effects, any connexion more intimate we have hitherto been unable to recognise In the dead house we have not unfrequently found circumscribed atrophy of the convolutions, which had not been pointed out during life by any decay of the supposed cerebral function, to which the atrophied convolutions should have subserved, and in far more numerous instances we have in vain sought for the atrophy, hypertrophy, anæmia, or hyperæmia of particular convolutions, in which one or other of these conditions ought to have existed in correspondence with some remarkable and pronounced mental affection We have thus tried the truth of the doctrine in both scales of the inductive balance, and both by the method of agreement and by the method of difference, it was found wanting

The accomplished advocates of phrenology would doubtless be able to shift off the burden of these objections, as they did when the skull of Swift was examined at Dublin, and found to be very wrong, they escaped by affirming that the shape of the poor Dean's head had been greatly changed by his long insanity But if in argument they slip through the ordinary rules of logic with such adroitness, they must not be surprised if their adversaries sometimes resort to the tropes of rhetoric, and use a little vituperative dirt, as fishermen take gravel in their hands to lay hold of eels

The doctrines of phrenology are irreconcilable with the received doctrines of the responsibility of man for his actions, and the witness who, in a court of law, assumes this mental philosophy to be the true one, assumes the folly, injustice, and worthlessness of the whole judicial system The judge might say to him, "If you are right, my occupation is gone it would be equally useless and unjust to hang that man for the consequences produced by the large cerebral organ behind his ear, do you take him, and endeavour by scientific treatment to remove the cerebral substance exuberant in that locality to the part where conscience and benevolence dwell As for me, like Prospero, I will bury my wig and drown my commission, the criminal law is a delusion and a snare, all right practice in law, polity, and religion, consists in scientifically operating on the organs of the brain"

In Simpson's 'Criminal Jurisprudence,' the doctrines of phrenology are applied in an honest and faithful spirit, and with a pitiless logic, to prove that the responsibility of a murderer is no more than the responsibility of a man with the gout

> "The doctrine of responsibility which appears to me to be alone consistent with reason religion and morality, is simply this,—that so far from the Creator having sent into the world some beings who are responsible, and others who are exempt from responsibility there is in fact no exception whatever, and that every human being is alike responsible—responsible (according to the degree of his departure, either in mind or body, from that degree of sanity necessary to the proper discharge of his social duties) *to undergo the painful but benevolent treatment which is requisite for his cure*"

This is the responsibility of the phrenologists, who appear to know that it is often easier to explain away a thing than a name, but it is not the responsibility of the common law, and is in fact quite a different thing If, therefore, the opinions of medical men on subjects of mental science, and on insanity, have been formed, if not in accordance with the whole details, at least with a leaning towards the doctrines of that system which results in this kind of responsibility, can we be surprised if they have found themselves in direct antagonism with the great functionaries of the law, the very essence of whose office assumes that man is answerable for, and punishable for, his actions in quite a different sense. We believe in the common law responsibility of man, though we do not use it as an argument against phrenology. If the latter be true, it will prevail, and when we find two truths apparently contradictory to each other, we can only, as moral observers, wait patiently for a new apocalypse to reconcile them. This corollary, however, we will venture to educe, that if judges, lawyers, and medical witnesses, could form similar, or at least not antagonistic, opinions on general questions of mental science, responsibility, free-will, and the like, criminal lunacy-trials would become much more satisfactory than they have hitherto been.

The next important modification of the lunatic theory is that which, without in any degree committing those who entertain it to the phrenological doctrines, refers all cases of insanity to some disease of the brain. It will be observed that this theory differs from the former one, not only in excluding the element of phrenology, but also in admitting that of *constant disease*. Insanity, according to the former, may sometimes be occasioned by unequal development of the organs, and consequent loss of balance without disease; according to the latter, an abnormal condition in the solids or fluids of the brain is essential. This opinion is so well supported by facts, by the authority of writers for whom we entertain the highest respect, and undoubtedly offers a true explanation in so many instances, that we question its universal truth with cautious reluctance. We doubt whether it can be either satisfactorily established or refuted, until we know something of the relationship existing between mind and matter. The physical and vital laws to which the cerebral organ is subjected, and the laws of mental action, come within the reach of our investigating faculties; but of the nature of their union we are entirely ignorant: their ultimate relation is "the adamantine wall against which the human intellect in vain beats itself."*

It is in vain that we attempt to solve this problem as it bears upon insanity, by the records of post mortem examinations. Our powers of observation do not always enable us to discover any alteration in the brain, even when insanity or death has been occasioned by the most obvious physical causes, by blows, insolation, or poisoned blood; and the testimony of most experienced pathologists goes to prove that the brains of insane persons present no uniform appearances dissimilar to those of persons dying from other chronic diseases, or from old age. This was Esquirol's opinion. Pinel thought that the anatomical lesions which he found, were merely the results of the malady, not, we suppose, as a solid lung is the direct result of inflammation, but as the wasting of a fakir's arm, which he has held behind him without using for a dozen years, is the indirect result of his mental condition.

It may be urged that if insanity occasioned by known physical causes leaves behind it no physical lesions with which we can identify it, but the existence of which lesions we are bound by the laws of the mind to admit, so also insanity produced by moral causes may invariably leave behind it cerebral lesions, which we are unable to detect on account of the imperfection of our senses

---

* Whewell.

Our want of necroscopic knowledge on these points, therefore, scarcely tells against the opinion that insanity is always a disease of the brain There is a want of evidence on both sides The only facts which appear decidedly opposed to the doctrine, are those not very unfrequent cases of insanity which, without any cause external to the mind, are gradually developed by the overgrowth of some predominant passion, as pride, fear, envy, ill-temper, vanity, generosity, or even the religious sentiment Such cases are not unfamiliar to physicians experienced in the treatment of mental disorders In such cases it may be possible to refer to some distant period, at which the patient was undoubtedly sane, but the mental change has been so gradual and insidious, that it is impossible to say when the insanity commenced As in Bayles' ingenious paradox of how many grains of barley make a heap, or how many drops of wine will make a man drunk, we may, if incautious, be compelled to the absurdity of allowing that one small increment of emotion can make the difference between sanity and insanity, between a healthy and a diseased brain

In such cases it is as impossible to fix the point at which sanity merged into insanity, as it would be to decide the precise time when a man has come under the influence of some ridiculous personal habit Indeed, these cases of insanity may be looked upon as instances of mental habits which have passed beyond the influence of the reason and the control of the will

An individual or his friends may perfectly remember the time when he was entirely free from some personal or mental habit The time may also be well remembered when the tendency to the formation of the habit existed, but when no difficulty and but little pain was experienced in checking its action and forbidding its indulgence, by exercising the coercive power of the will And a third period may be easily fixed upon, when the man has become the abject slave of the habit—locked into it and carrying it about with him as a Chinaman does his neck-stocks Contrary to the opinion of the Rev Sidney Smith, it appears to us extremely difficult to fix the period when the habit is formed—whether, in the case of a personal habit, for example, it may be said to have established itself at the fourth, the fortieth, or the four thousandth repetition This author has also perverted the habitual use of the term, in applying it to those deep marks left in the mind by the rude and rapid inroads of emotion, so humorously illustrated by him in the formation of Dr Isaac Barrow's habitual dislike of dogs, proceeding from a tussle between the mathematician and a mastiff, to which it is added that " the adventure gave the doctor a strong habitual aversion to dogs, and I dare say, if the truth were known, fixed in the dog's mind a still stronger aversion to doctors "

This application of the term does not appear to be in accordance with its conventional usage Locke, referring to confidence in speaking, says "which power or ability in man of doing anything when it has been acquired by *frequent* doing the same thing, is that idea we name *habit*, when it is froward and ready on every occasion to break into action, we call it *disposition*." (b. ii. c. 22.) Butler, who uses the term in a wide sense, and speaks of habits of virtue, moral and religious habits, &c., does not extend it to comprehend single but permanent impressions, like Dr. Barrow's canine aversion. He uses it in a manner consistent with our views of its bearing on pathology, thus:

> "For distinctiveness, we may consider habits as belonging to the body or the mind, and the latter will be explained by the former. Under the former are comprehended all bodily activities or motions, whether graceful or unbecoming, which are owing to use; under the latter, general habits of life and conduct, such as those of obedience and submission to authority, or to any particular person; those of veracity, justice, and charity; those of attention, industry, self-government, envy, revenge. And habits of this kind seem produced by repeated acts as well as the former." (c. v.)

Whether the alteration of temper and character produced by the habitual despotism of some particular emotion or set of emotions be capable of causing insanity, can be determined only by observation of instances. According to our own experience, insanity thus produced is by no means unfrequent, and many cases may be cited, from Esquirol and other authors, which are clearly deducible from defective education, and the compelling force of some bad habit of mind. We believe that a sound, moral, and religious education would not only prevent an immense amount of crime, but also a great proportion of that insanity, whose prevalence stigmatizes an uneducated civilization. Like Kavanagh's schoolmaster, we may fairly speculate "on what Lady Macbeth might have been, if her energies had been properly directed."

Some of the meaner emotions are peculiarly liable, like dry rot, to destroy the stability of mental health. We have observed excessive vanity, selfish timidity, propensities to anger, envy, and covetousness terminating in this manner, by overgrowing and preventing the free actions of the other faculties. Habits of mind of a passive nature lead more directly than active ones to this melancholy result; thus the disposition to be unduly moved by the trivial annoyances of life, if it causes the resistance and the habit of peevishness, never exceeds a certain limit, which, although it may render the subject of it very ineligible for a club or a mess-room, does not entitle him to the benefits of an asylum; whereas, if the same disposition exists without causing resistance, but occasioning only disquiet and low spirits, it is in a fair way to lead its victim to helpless dejection, melancholy, and insanity.

Our active habits are bounded and limited by the external resistance they meet with; our passive habits are unlimited, and, like the tub of the Danaides, can never be filled. It should be observed, that excessive emotions thus overgrowing and dominating the mind, cannot correctly be said to *cause* insanity, as sudden fright or grief will cause an attack of mania. Properly speaking, they *constitute* the disease. Thus, checked perspiration causes rheumatic fever, but an excessive quantity of blood, or an accumulation of fat, constitute abnormal conditions of the human frame, which impede the due performance of the functions, and are practically recognised as serious diseases.

If, therefore, it be admitted that the emotions and passions, gradually acquiring an inordinate power, and carried to an undue extent, are capable of constituting insanity, it is clear either that insanity may exist without disease of the brain, or that disease of the brain must exist whenever any of the emotions are in excess The latter assumption resolves itself into an absurdity, or into a mere quibble, on the meaning of the word disease

Having thus briefly referred to the general principles of the somatic theories, we cannot at the present time examine the subdivisions or separate articles of these creeds, or discuss whether anæmia or hyperæmia of the cortical substance, or meninges, or poisoned blood, or peripheral irritation, or "want of tone," are the most potent causes We must proceed to give such consideration as our limits will permit, to the most important psychical views on this question

John Charles Bucknill

(To be continued)

Review XIII

Norsk Magazin for Lægevidenskaben Heft 1—10 1852
Norse Magazine for Medical Science Parts 1 to 10 —*Christiania*, 1852

This medical journal is, we believe, the only one published in Norway, and appears in monthly parts, under the sanction of the Medical Society of Christiania Each part contains some original articles, followed by extracts from various Continental and British journals and concludes by a résumé of the proceedings of the monthly meetings of the Medical Society Notices are likewise given of all changes in the laws affecting the medical profession in Norway, and to judge from their number and variety, the healing art is deemed of more importance to the welfare of the state in Scandinavia than in our own land We purpose here briefly to analyze the more important original communications in this journal, as they afford much insight into the diseases and the sanitary condition of the remote portion of Europe to which they relate Some of these papers, however, will hardly admit of satisfactory condensation, and there is a large amount of interesting matter contained in these numbers, which must, for want of space, be omitted in our notice

Dr Danielsen's report of the cases that have been treated in Lungegaard's Hospital in Bergen, during the years 1850 and 1851 (Nos I and VII), is an able document, not only for the diligence with which the various diseases there occurring have been observed, but for the new modes of treatment which have been cautiously yet perseveringly pursued

The hospital in question is principally devoted to the reception of that form of elephantiasis termed in Norway "Spedalskhed," and which occurs under the name of "liktraa" in Iceland, and in more southern climes is known as Elephantiasis Græcorum As this malady has been more than once described at considerable length, in the pages of this Review,* we shall here only notice the further researches of Dr Danielsen on this interesting subject, and the novel modes of treatment that he has pursued with varied success

In the year 1850, 95 patients were treated in this hospital, 70 of whom laboured under the disease in question Fifty of these patients were affected with elephantiasis tuberculosa, 11 had elephantiasis anæsthetos, and 9 suffered from both forms of the malady. The remaining 25 were cases of syphilis, or of ordinary skin diseases, and most of these left the hospital cured. So favourable a result was, however, not obtained in regard to the patients affected with spedalskhed. Out of the whole 70 cases, only one was cured, and in him the malady can hardly be said to have developed itself, 3 left the hospital improved in health, 5 more went out without having experienced the least amendment of their condition, and 4 died. Many of the patients, it appears, reside in the hospital at their own expense, and are consequently obliged to leave it when their scanty means are exhausted. Of the 3 who went out improved in health, 1 was a girl of 21 years of age, affected with hereditary spedalskhed. After ten months of treatment, the tuberculous masses, which covered, more or less, the whole cutaneous surface of the body, had almost entirely disappeared, but as she still exhibited other symptoms, which showed that the morbid diathesis was not entirely eradicated, Dr Danielsen did not place her on the list of cures. The second, a young man of 20, had the disease to such a degree, that every part of his body was covered with tuberculous masses, which in many places had united together, so that the surface was enormously swollen and disfigured. In seven months, these symptoms had, under appropriate treatment, almost entirely disappeared, he left the hospital to all appearance well, and remained at home for six months after in good health. In December, 1850, he made a voyage in an open boat to Bergen, and suffered much on that occasion from exposure to cold, the disease again broke out, and he re-entered the hospital.

---

* See review of Danielsen and Boeck on Spedalskhed, in vol v

Of the 4 who died, only 1 can be said to have fallen a victim to the disease itself. In this individual a great many of the subcutaneous tubercles softened in succession, and formed large ulcerating surfaces, which gradually exhausted his strength. Two others died of chronic diarrhœa, which came on about a fortnight after the protracted employment of tartrate of antimony in gradually increased doses, but no pustular eruption was found on the mucous membrane of the intestines on dissection. The fourth patient died from ulceration of the mucous membrane of the bowels, and in all, the tuberculated form of elephantiasis existed, though in two it was complicated with anæsthesia of the extremities. The first of these two individuals had been for four years a sufferer from the malady in its tubercular form, and large tracts of the skin were infiltrated with tuberculous matter. A few days before his reception into the hospital, he began to complain of intense pain in the right eyeball, with diminished vision and some intolerance of light. The other eye was soon after affected in like manner, and about the same time, severe pains were felt in the hands and feet, extending to the arms and legs, with excessive sensibility of the skin of these parts. Antiphlogistic treatment relieved these symptoms for a time, but the affection of the eyes returned with increased violence, and the intolerance of light became extreme. The morbid sensibility of the extremities now disappeared, and was replaced by complete anæsthesia of these parts. Death at length ensued from softening and ulceration of the tuberculous masses beneath the skin. On examination of the head, it was found that a thick, firm exudation existed in the arachnoid membrane, directly over and pressing upon the decussation of the optic nerves The ulnar and tibial nerves were carefully dissected out, and in certain parts of their course they were observed to be swollen and hard to the touch, and the sheaths of the nerves were of a reddish colour from the distended bloodvessels they contained On making a transverse section of these nerves, it was observed that here and there yellowish-white tuberculous masses were infiltrated between the nerve-tubes, by which the size of the nerve was considerably augmented Dr Danielsen observes that amaurosis, which occurred in this case for some time before death, has been found, by Dr Turk of Vienna, to arise often from a chronic exudative process of inflammation, by which the mass of the optic nerve is infiltrated, compressed, and rendered unserviceable Here, however, the nerve was unaltered, but the deposit on the arachnoid probably compressed it, and produced the same effect

In the second case pain was felt in the right ear, and the hearing on that side was totally lost On dissection, a neuroma was found situated on the right auditory nerve, immediately before it enters the temporal bone, and compressing the fibres of the nerve so as to reduce them to an atrophied condition The same appearances as before described were observed in the nerves of the extremities

Dr Danielsen believes that where anæsthesia of the extremities comes on in the course of spedalskhed it is the result of inflammation of the sheaths of the nerves, indicated by the excessive sensibility of the skin, and this is followed by the deposit of the whitish yellow albuminous matter between the fibrils of the nerves, compressing them, and at length producing total loss of feeling in the parts to which the nerves are distributed When, as sometimes happened, the cutaneous nerves were inflamed in this manner, they could be felt as hardened cords beneath the skin, and where there was hyperæsthesia of the extremities, as for instance of the hands, the ulnar nerve at the elbow was so extremely sensible, that the patient almost fainted with pain when it was slightly compressed Dr Danielsen regards these symptoms as of great importance in a therapeutic point of view, as the favourable moment for early and effective treatment soon passes away, and the attention of the practitioner should be sedulously directed to the subduing of the inflammation, so that the deposit of albuminous matter in the nerves, and the consequent anæsthesia may not be produced For this purpose local treatment by cupping, &c, is absolutely necessary, and the results obtained by these means have been very encouraging Local treatment has, however, no influence on the morbid diathesis which constitutes the disease

Dr Danielsen has satisfied himself that in spedalskhed there is an excess of albumen and fibrin in the blood, and against this as the *materies morbi*, his treatment has been chiefly directed With this view the diet of the patient was restricted chiefly to vegetables, and to excite the secretions of the skin, steam baths were given every second day Internally, phosphoric acid, tartrate of antimony, iodide of potass, chloride of gold, and chlorine, were given, with variable results Phosphoric acid, largely diluted with water, produced no good effects, though its use was long persevered in Tartrate of antimony, especially when it was had recourse to in an early stage of the malady, diminished and rendered more regular the superficial circulation, but when long employed, it caused, almost invariably, obstinate diarrhœa During its use, it was always remarked that the tubercles of the skin diminished in size, and became of a paler colour Iodide of potass was given chiefly in the tubercular form of elephantiasis, iodide of iron was principally used in the anæsthetic variety, and no medicines produced results so favourable as these

> "Where the nodose (tuberculated) form exists to a high degree, and the system has suffered for years from the altered condition of the blood, the iodide of potass must be given with caution It is liable, after having been administered for several weeks in succession, to cause great congestion of the skin, which becomes then of a deep crimson colour, swells, and is very painful, and if you then pass the hand over the surface of the patient's body, you feel, deep below the skin, many hard masses of various dimensions which seem to have been formed within a day or two The older tuberculous masses become redder and swell up, and a general febrile exacerbation accompanies these symptoms Great pain is now felt along the course of the ulnar and tibial nerves, extending downwards to the fingers and toes The above mentioned nerves are found on examination to be hard, swelled, and extremely sensible The eyes are often much affected, congestion of the conjunctiva, or exudation of lymph into the pupil, frequently occurs All these symptoms, however rapidly disappear if nitre be given in large doses (gr xv ad ʒss) every second or third hour, and at the same time blood should be taken by cupping Upon the cessation of these untoward symptoms it is generally found that the tubercular masses have greatly diminished in volume, that the skin has become soft and elastic, and more nearly approaches its normal condition Occasionally the iodide of potassium is well borne for two or three months together, and during this time the tubercles decrease and assume a bluish colour, while most profuse perspiration ensues, and continues for three or four weeks without materially impairing the patient's strength and during this time the amendment advances as before"

The iodide of iron acts more slowly, it is particularly useful in those cases where anæsthesia is present

Dr Danielsen often allows an interval of two or three months to elapse, and then resumes his former mode of treatment, and he has remarked that the second course of the iodides of potass or of iron is less frequently injurious, and produces more beneficial effects than the first

The chloride of gold was tried both in 1850 and 1851, but with completely negative results In 1851,* 85 patients affected with spedalskhed were admitted into the hospital, 57 of these were males, and 25 females 52 of the whole had the tubercular form of the disease, 11 the anæsthetic, 8 the mixed form, 13 had maculæ elephantoideæ, and 1 had only the prodromata of the disorder 3 were cured, 3 more were improved in health, 18 left the hospital in the same condition as when they entered it, and 2 died All the 3 cases that were cured, presented the disease in its earliest stage One of these patients was treated with arsenic in small doses, but Dr Danielsen does not speak favourably of this potent mineral poison as a remedy in spedalskhed Indeed, in advanced cases, he considers it to be positively injurious, producing, gradually, its poisonous effects without destroying the disposition to elephantiasis In the tubercular form, however, where the deposits in the skin have a dark brown or black colour (morphea nigri), the employment of arsenic is often beneficial

The only new remedy tried extensively in 1851, seems to have been oxalic acid Dr Danielsen was led to its employment by its asserted effect on the constituents of the blood, and on the process of digestion We are not yet informed of the results produced by this remedy, as regards the cure or amelioration of the disorder for which it was administered, but Dr Danielsen s experience of it has enabled him to correct many of the erroneous ideas entertained of its poisonous properties It has generally been stated, that in doses of ʒij and upwards, its poisonous effects are rapidly produced on the human system, and that it causes then paralysis of the heart, with almost immediate death *

---

* See No 8

Dr Danielsen dissolved pure oxalic acid in 10½ parts of water, and began his treatment by administering ten drops of this solution in a tablespoonful of water every two hours Every five days, five drops more of this solution were administered, until 150 drops were taken every second hour One patient took this large quantity without the addition of any water for upwards of a month, and not till after that time did she complain of any burning sensation in the fauces, while this was speedily removed by her taking the medicine in a little gruel Eleven patients, aged from 10 to 50 years, were treated in 1851 with oxalic acid, and no particular effects were observed in any of them until they took 40 or 50 drops, or 7 to 8 grains of pure oxalic acid, every two hours, when in some the pulse sank down to seventy beats per minute, but they complained of no uneasy sensations One girl, aged 15, continued the use of the remedy for eleven months, and during the last three months of that period, she took daily ʒij to ʒijss of pure oxalic acid, without the slightest injurious effects During this time, the appetite continued good, and the bowels were perfectly regular, while her general health was much improved The great affinity of oxalic acid for lime, and the consequent formation of a compound insoluble in water, made Dr Danielsen fear that such a combination might be formed within the body, and that thus the operation of the remedy might be rendered abortive He therefore submitted to careful analysis the whole of the urine passed during three days, by the patient who was taking daily ʒijss of pure oxalic acid During the three days that the urine was collected, this patient had taken very nearly an ounce of the acid, but only two grains could be discovered in the urine, or about $\frac{1}{105}$ part of the whole Nor was the acid passed in any state of combination by stool, for the fæces of two days yielded hardly one grain of oxalic acid'

Dr Danielsen confesses that the progress hitherto made in the treatment of spedalskhed is far from encouraging, but he is evidently pursuing the right course of careful observation and experiment, and we may hope that this will eventually be crowned with success The most serious obstacle to effecting a perfect cure in these cases of spedalskhed, is the length of time required for the employment of remedies, most of the patients are poor, their scanty means of payment are exhausted, and they must then return to their homes, as the institution does not appear to possess funds for their gratuitous support We rejoice, however, to learn that the Stor-

---

* We believe that half an ounce is the smallest dose that is known to have proved fatal to the human subject but the case related by Dr Scott of Cupar (Edinburgh Medical and Surgical Journal, vol xxiv p 67) where a girl swallowed about a wineglassful or nearly two drachms of oxalic acid proves that even in that dose it acts most powerfully on the system It is however by no means certain that a larger quantity was not swallowed in this instance We are inclined to doubt the truth of the assertion of Henke and Most that oxalic acid is most dangerous when it is diluted with large quantities of water When concentrated it acts first as an irritant poison ; when much diluted its effects are exhibited on the nervous system but not till after some time

thing of 1850 voted funds for the building and support of a large hospital at Bergen

There is an able article in the first number by one of the editors of this journal, Dr Faye, on the relative value of the separate and of the silent systems (Auburn and Philadelphia systems) of prison discipline Dr Faye has studied the operation of these systems in England and elsewhere, and he is evidently inclined to prefer the separate or Philadelphia plan to that where silence is enforced, but the prisoners work in a common room He is of opinion, however, that term of seclusion should not be too much extended, that no exertion should be spared to excite during this period the moral and religious feelings of the criminal, and above all, that means should be devised for enabling the reformed prisoner to regain, in part at least, the social position he has forfeited We have not space for any lengthened analysis of this sensible paper, but at its conclusion, we learn what has been done in the north of Europe towards a reform of prison discipline in accordance with these views

In Denmark, the American system was adopted as a principle in 1842, and it was determined that the separated plan of prison discipline should be followed for a brief term of punishment, and the congregated but silent system for a longer period The new prison at Horsens is to be fitted up on the above plan, but our author does not know if it be already in operation

In Sweden, the prisons for the different provinces (*Lansfaengsler*) have been provided with cells for the separate system, and this plan of discipline will be chiefly adopted for the suspected criminals (*Ransakningsfangar*), so as to save them from the deleterious society of the older and more hardened criminals

In Norway, a different mode of proceeding has been adopted, and a large prison was erected at Christiania, on the separate system, in 1852 The shortest period of solitary confinement in the building is to be three months, the longest four years

In the second and fifth numbers of the journal, Professor W Boeck presents us with a well written retrospect of the more remarkable cases of skin disease, and of their treatment, in the two wards allotted for this purpose in the Christiania hospital Dr Boeck adheres to Cazenave's nomenclature and arrangement, though he confesses that the natural system, founded on anatomy and physiology, and promulgated by Erasmus Wilson in London, and by Hebra in Vienna, is the more scientific, and will, in the end, be generally adopted In scabies, for example, the insufficiency of the old artificial system is very apparent Scabies is there placed amongst the vesicular diseases, because the acarus scabiei most frequently produces vesicles, but the disease may exist without a single vesicle, and papulæ, impetiginous pustules, ecthyma, and furunculi, may also be present Dr Boeck details a curious case of scabies in a young girl of 15, where the inner surfaces of the hands and fingers were densely covered with thick, whitish-grey, shining crusts, so closely agglomerated, that they could be peeled off like a piece of bark The same condition of the skin was observed on the soles of the feet, on the nates, the elbows, and on portions of the back Flat, brownish red scabs were observed on the shin-bones, on the upper surface of the arms there were many vesicles, and here and there on the extremities a few pustules The disease had lasted for about two years When the patient was put into a warm bath, the crusts fell off, and the moist, red, uneven skin appeared On examining these crusts with the microscope, they were found to consist almost entirely of sarcoptes scabiei, there were entire insects, with their eggs, shells, and excrements, but only one living specimen could be detected The malady was highly contagious, and the nurses and the patients inhabiting the same room were all more or less affected After warm baths had been employed for some time to loosen the crusts, the Vienna ointment (pulv cretæ alb ʒss, flore sulph et picis liquid āā ʒvj, sapon virid et axungi āā ℥jss) was applied to the whole body The crusts now rapidly disappeared, but about three weeks afterwards, a pustular eruption appeared over the whole body, and even on the face, while around these pustules small crusts were formed like those before described On submitting them to the microscope, it was found that they consisted, externally, of a shining layer of epidermis cells, and beneath this was a grey-coloured layer of the remnants of sarcoptes scabiei A further use of the Vienna ointment, with warm baths, freed the patient entirely from this disgusting malady The rest of this long article is occupied by a clinical disquisition on favus and its varieties, and on the necessity of microscopical examination to determine the true character of these diseases of the hairy scalp Dr Boeck has always found true favus to be characterized by a fungous parasitic growth, by the sporules of which he believes that the malady is propagated He considers the eradication of the diseased hairs to be an essential requisite for effecting a permanent cure

In the continuation of his clinical remarks in the fifth number, Dr Boeck treats chiefly of gonorrhœa and syphilis He believes that radesyge, a disorder so frequent in Norway, and more than once described in this journal, is nothing more than an after product (*Affødning*) of syphilis, and he maintains that the disorder was first introduced into the country by the sailors of a Russian ship of war, about the year 1709 The historical details he has collected to corroborate this opinion are extremely curious, and we do not remember to have seen them alluded to in any of the numerous works that have been published on radesyge

From a report on typhoid fever in the Christiania hospital, by J W Randers, contained in the third number of the journal we learn that the well-known lesion of the glands of Peyer is generally present in this disease as it occurs in Norway, but that cases of well marked typhoid fever are not unfrequently met with there, where these glands are totally unaffected In the same number, Professor Boeck details an interesting case of the cure of aneurism by compression, after the method practised by Mr O B Bellingham and others After trying to effect adequate compression of the femoral artery by the ordinary means without success, owing to the pain thus produced, he at length obtained the apparatus proposed by Mr Bellingham, and found it to answer his most sanguine expectations A small stream of blood was allowed to flow through the aneurism, the pulsation in the tumour diminished, first in the upper part, and then in the lower, when the patient was attacked about the tenth day with inflammation of the right lung, and the compress was laid aside on the twelfth day, at his urgent request At this time, pulsation could still be distinctly felt in the lower part of the tumour, but on examination the following day, it could not be detected, nor was there any movement in the femoral artery for several inches above the tumour, or in the tibial arteries below Three days after, the compress was again applied, and in four days more the cure seemed to be completed Tubercular disease of the lungs now appeared, and the patient died, after the lapse of six weeks from that time A careful examination of the limb was made after the arteries had been previously injected No collateral circulation had been established, the smaller arteries were not dilated, but the circulation had been carried on through the femoral artery by a very narrow canal, which passed through the much diminished aneurismal swelling

A partially successful case of Cæsarean section is recorded in No 6 by Dr Faye The pelvic diameter was narrowed by two large fibrous tumours depending by long stalks from the sacro iliac fossa on either side into the cavity of the pelvis, and reducing the space for the extraction of the infant to little more than an inch Each of these tumours was as large as the head of a seven-months' fœtus The operation was delayed for a day or two, in the hope that some softening or displacement of these obstacles might ensue, and because the pains were not severe, but at length it was determined to extract by the Cæsarean section, as the child was still living, and the os uteri could not be reached, for it was pushed high up towards the brim of the pelvis by the adjacent tumours Even if the os uteri could have been reached, the extraction of the child piecemeal would have been impracticable through the narrow space of one inch that was available The operation itself presented no difficulties, chloroform was used, and a living healthy child was brought into the world Symptoms of peritonitis set in on the following day, and the patient died thirty hours after the operation Dr Faye thinks that it would have been impossible to push the tumours upwards into the cavity of the abdomen, or to reach the neck of the tumour from below, so as to be able to excise them ith safety Considerable hemorrhage ensued after the placenta was removed, it was immediately controlled by compression of the aorta through abdominal parietes The advantages of this mode of arresting terine hæmorrhage have been, as Dr Faye remarks, much discussed of late, both at Paris and at Brussels It has been maintained that the hæmorrhage arises chiefly from the uterine veins, and that the vena cava is thus emptied by a reflux of the blood it contains, while the 'arteriæ ovaricæ" may still continue to supply blood to the uterus, as they originate above the point where compression is generally applied As the origin of these arteries is on the right side, generally from the arteria renalis, and on the left from the aorta itself at about the same level, Dr Faye has always thought it necessary to compress the aorta some distance above the umbilicus, instead of directly over that point

The Cæsarean section has been performed in Norway four times within the last ten years In two of these cases it was had recourse to in the last extremity, and after the head of the child had been already perforated, and in both these instances the mothers expired shortly after the operation, completely worn out by their previous sufferings In the third case, and in this present instance, the children were saved, but the mothers perished

In the same number (6) we find given at full length the extraordinary trial for poisoning with lobelia inflata, which took place a year or two ago at the Cumberland assizes, before Judge Patteson. Our readers will probably recollect the humiliating treatment experienced by the medical witnesses, from the judge and from the counsel for the defendant (Serjeant Wilkins), and the incapacity of a jury of Cumberland farmers to decide such a question is here strongly insisted upon by the reviewer.

The seventh number contains some valuable remarks on puerperal fever, by Dr Faye. He adopts, in its fullest signification, the modern doctrine, that puerperal fever in all its forms is to be regarded as a phlebitis, or an inflammation of an unhealthy character, and insists strongly on the highly contagious nature of the disease. In 1850 an epidemic of puerperal fever prevailed in the lying-in wards of the Christiania hospital, but this abated rapidly after careful prophylactic measures against contagion were adopted. The utmost cleanliness on the part of the attendants was enjoined, the hands of the operators were constantly washed with chloride of lime, and even the oil used for examinations was mixed with disinfecting substances. (Would it not have been better to have employed fresh oil for each examination?) To strengthen the constitution of the patients, and to preserve them from liability to infection, sulphate of quinine was given in full doses at an early period. The spread of this malady, like many others, by a process analogous to that of fermentation, from a small amount of poison introduced into the system, is corroborated by Dr Faye's observations. His treatment of this zymotic disease is grounded on the hope of producing a total change in the condition of the blood, by large and repeated doses of calomel, after the method proposed by Ritgen and others, but he considers this practice to be only advisable where the treatment is commenced on the first appearance of symptoms of the disease. Twenty grains of calomel were given once or twice daily, and followed by the exhibition of ʒss. of castor oil, every three or four hours. In 1850 Dr Faye had hesitated to employ purgatives until at least two days had elapsed from the period of delivery, but he found, from bitter experience, that if purgatives were not employed at the beginning, the favourable time for their administration soon passed by, and the bowels became so paralyzed by peritoneal inflammation, that they could not be induced to act by the strongest evacuants. Quinine and opium were likewise given along with calomel in large doses, from the first day that the fever showed itself. Dr Faye refers to Mr Garlick's treatment of puerperal fever as corroborating his practice of giving laxatives at an early period in this disorder. He remarks, too, that although so large a quantity of castor oil was given, the secretion of milk was rather increased than diminished, and he suggests that this oil may really have the property of augmenting the lacteal secretion, as is stated by Dr McWilliam, in the 'Monthly Journal of Medical Science,' for October, 1850. We are glad to observe that at every meeting of the Medical Society of Christiania, the "constitutio epidemica" of the town, and of the country in general, is made a subject of discussion and report.

At the end of the tenth number a list is given of the subscribers throughout Norway to the fund for a monument to Jenner. The sum that has been thus collected (240 Sp. D.) will, perhaps, appear small, but

Norway is not a land abounding in wealth, while the medical practitioners are but indifferently remunerated for their labours. Still, it is pleasing to observe that, out of their scanty means, our Norwegian brethren of the profession have contributed so large a proportion of the whole amount that has been raised for this purpose throughout that kingdom.

It will be seen, from the brief analysis we have here given of some of the more generally interesting papers, that the 'Norway Medical Journal' is carried on with considerable spirit, embracing every department of the profession, and discussing its various subjects with an earnestness and impartiality well worthy of imitation. We could indeed wish to receive more frequently separate works or monographs of particular disorders, from our Norway brethren; but next to this we rejoice to observe that the discoveries of other countries are readily received and appreciated by the members of the Medical Society of Christiania.

Edward Charlton.

Review XIV.

1. *A Treatise on Tuberculosis, the Constitutional Origin of Consumption and Scrofula.* By Henry Ancell, late Surgeon to the Western General Dispensary, and Lecturer on Materia Medica and Medical Jurisprudence, &c. &c. London, 1852. 8vo, pp. 779.
2. *The Nature, Symptoms, and Treatment of Consumption; being the Essay to which was awarded the Fothergillian Gold Medal of the Medical Society of London.* By Richard Payne Cotton, M.D., Member of the Royal College of Physicians, and Assistant-Physician to the Hospital for Consumption and Diseases of the Chest. London, 1852. 8vo, pp. 286.

In an article contained in the twenty-first number of this Journal (January, 1853) we classified and gave an abstract of the views entertained by various pathologists as to the structure and pathological origin of tubercle, and stated the question at issue as to what disease or diseases should be signified by the term phthisis. The authors whose works are placed at the head of this article belong to that section of pathologists which, as we then stated, has for its most recent exponent, Lebert.

Tubercle is with them a secretion, or rather, a developed secretion;—for the tubercle-blastema is in reality the secretion, and the tubercle the same blastema, organized to a certain extent by the development in it of tubercle corpuscles. The tubercle-blastema is derived directly, like all other secretions, from the blood; and it is in this fluid that Mr. Ancell looks to find the primary, the essential disease. Tuberculosis is, he states, "neither a cachexia nor a diathesis, but an idiopathic malady affecting the blood." And again: "Tuberculosis will in this volume be regarded as an idiopathic blood-disease." Dr. Cotton speaks more vaguely as to the primary affection, observing merely that it is constitutional; still he evidently holds that the blood must be diseased before tubercular matter is formed, for he speaks of this matter being, in a large majority of cases, merely separated from the blood. "Tubercular matter is formed in two ways: 1st. By a gradual and uninflammatory separation from the blood; 2nd. By inflammatory action ending in a morbid scrofulous secretion, instead of a healthy fibrinous one

The first of these is by far the most common." The constitutional state and the blood-disease to which it necessarily leads are designated Phthisis, or Consumption, by Dr Cotton. But he does not always use these words in the sense he has himself assigned to them at the outset of his work, for at p. 12 he uses the word phthisis to signify the local disease, while at various parts of his essay he appears to use phthisis and consumption indifferently to express the general constitutional state, the blood disease, and the one of the local lesions which is the consequence of the blood-disease—i. e., tubercles in the lungs.

The Fothergillian gold medal of the Medical Society of London was awarded to Dr Cotton's essay. It is divided into three parts, the first of which treats of the *nature* of consumption, the second, of the *symptoms* of consumption, and the third, of the *treatment* of consumption. It contains the expression of his own opinions as to the first of these points, and the result of his own observation at the Brompton Hospital—expressed, with one or two exceptions, in the most general terms—in regard of the last two.

Mr Ancell's work is an epitome of all that has been done and said in reference to scrofula and tubercle, excepting those views of Virchow, Van der Kolk, &c., which were analyzed in the late numbers of this journal.

Mr Ancell considers his subject under eight heads:

1. Tuberculous predisposition.
2. The signs and symptoms of tuberculosis.
3. The history of tuberculous deposits.
4. The special pathological anatomy of tuberculosis.
5. The causes of tuberculosis.
6. The general pathology and essential nature of tuberculosis.
7. The forms and varieties of tuberculous diseases.
8. The treatment of tuberculosis.

To each of these subjects a chapter is devoted.

Of the Tuberculous Predisposition.—That condition of the system, or rather those individual peculiarities of form which are ordinarily said to indicate a predisposition to tubercular disease, are considered by Mr Ancell to have their origin in, and to be the result of, the blood disease which he calls tuberculosis. Holding that tuberculosis always precedes the deposit of tubercle, and that this blood-disease is the primary departure from health in every case in which a deposit of tubercle occurs, Mr Ancell opens his work with an attempt to determine from the observations of others, in regard of the general physical, the microscopical, and the chemical characters of the blood in the very outset of tuberculosis, what is the essential deviation from health in that affection. Thirteen pages are devoted to this subject, but the greater part of the so called facts thus collected together are opposed to each other, and the opinions of the writers quoted are almost as numerous as the writers themselves. Some—as Baumes and Denis—affirm that the clot in blood drawn from scrofulous individuals is large and soft, and coagulation rapid, others, on the contrary—as Dubois and Andral—that the clot in such cases is small and dense, and that coagulation takes place slowly. As to the microscopical observations hitherto made on the blood of scrofulous subjects, they are utterly worthless. Appearances which may be produced at will in the blood of the most healthy are set down as signs of disease, and no evidence is afforded that the various sources of error, so difficult to avoid as to deter the most accomplished microscopical observers from entering on the subject, were shunned.

As to the chemical constitution of scrofulous blood, L'Heritier states, that in scrofula the earthy salts are diminished, Fricke, that they are increased; the fat elements are by some held to be above the average, by others to be below the mean of health. The fibrine has been found to be very nearly as often in excess, as deficient in quantity; and the only points that appear established by the many chemical analyses made of the blood of scrofulous and tuberculous subjects—of the blood, that is to say, in tuberculosis—are, that the blood-discs are deficient in number, and that the albumen is increased in quantity. But the fibrine and the albumen are defective in quality: their vitality is diminished, in the opinion of Mr. Ancell; and as this defect in quality of the fibrine and albumen is the centre-point of all Mr. Ancell's opinions—the basis of his pathology of tubercle—we shall give, in his own words, his explanation of the term.

> "In living matter the degree of vitality is measured by the sum of vital actions, comprising, not only chemical and physical changes produced under the influence of the vital force, but actions referrible to the vital properties of irritability and sensibility. The healthy constitution of man corresponds with a physiological range of vital actions in the blood and the living tissues, but especially in the blood. These actions, as respects the blood, consist in all the molecular changes which take place in hæmatosis. They comprise those which result from the dynamic property of endosmose and exosmose in the corpuscle, as well as those which mark—its organic contractility—the assimilation of old and new matter to the form of blood—the formation of red corpuscles and liquor sanguinis—the consumption of red corpuscles—and the waste of liquor sanguinis in nutrition. When the sum of these molecular actions is within the physiological range, and they are in harmony with each other, a sufficient number of corpuscles being formed and wasted, and all the constituents of the liquor sanguinis being atomically complete and of normal proportion, and consumed and renovated in proportion to the requirements of the living structures, the blood has a healthy degree of vitality; but in tuberculosis the sum of these actions is below the physiological range, and moreover the proportions and qualities of the blood are deranged. This marks a low degree of vitality, which, as we shall see, is consistent with all the phenomena of the tuberculous predisposition, and with the signs and symptoms of tuberculosis in its various forms." (p. 15.)

For our part, after giving this paragraph as attentive a consideration as we are able, we must own that some parts of it lie beyond our comprehension, and that we are unable to see, adopting Mr. Ancell's own definition of diminished vitality in regard of the blood, that in tuberculosis the *blood is deficient in vitality*, for the only constituent of the blood deficient in quantity is the red blood discs, while the albumen is in excess. And with reference to the blood discs, although deficient in number, there is no evidence before us to prove that they are defective in quality; for the microscopical observations are, we repeat, judging from their intrinsic evidence, absolutely worthless; and we may add, our observations, more numerous than those he has quoted, fully justify this *à priori* conclusion.

Of the condition of the *lymph* and *chyle* in tuberculosis, literally nothing is known.

While certain physical deviations from a healthy standard are said by many to indicate a predisposition to tubercular deposition — e g, the smooth, soft, delicate, and transparent skin, through which the bloodvessels are perceptible to the eye—Mr Ancell, who holds these to be evidence of the existence of tuberculosis, of an already established blood-disease, regards these deviations from healthy structure as the result of the evolution of blastemata of defective vitality He argues that, the blood being healthy, healthily-constituted blastemata are formed by it, which develop into normal tissues But in tuberculosis the blood is diseased, hence the blastemata poured out from that blood are diseased, and as a consequence, the structures formed from them are defective in constitution

Mr Ancell then passes in review "the cutaneous system" and its appendages, the mucous membranes, the teeth, the vascular system, the serous membranes, the osseous tissue and the skeleton, the muscular system, the viscera, the lymphatic glands, and the organs of special sense, as these are severally modified in structure by the circulation of tuberculous blood, and describes the deviations from their normal functions manifested in the digestive, respiratory, circulatory, secretory, muscular, sensorial, moral and intellectual, and generative organs Dr Todd's opinions in reference to strumous dyspepsia are adopted by Mr Ancell With reference to the relation which exists between deformities of the chest and tubercular disease, Mr Ancell remarks

> "The contraction of the chest, and the defective structure and functions of the lungs have been properly regarded, as long as the tuberculous state of the blood exists as a real predisposing cause of the development of tubercles in the lungs, but they have also been regarded, without any conclusive evidence, as the cause of the tuberculous cachexia—that is to say, of the disease of the blood This subject will have to be considered in a future chapter it is sufficient here to observe, that the mal nutrition of the lungs, and of the bony parietes of the chest, is of the same nature as that which is liable to pervade any other part of the system, that tuberculosis pulmonalis is but one form of tubercular disease, and that if we were to admit this principle of etiology, we must also, by a parity of reasoning admit, that where tuberculosis meningitis exists without any pectoral predisposition or disease, the irregular formation of the skull and mal nutrition of the membranes of the brain constitute the cause of the disease of the blood' (p 42)

The *muscular powers* are deficient in those suffering from tuberculosis This loss of power is witnessed not only in the voluntary, but also in the involuntary muscles The latter is exhibited in the diminished power of the heart, the feebleness and distensibility of the muscular coats of the intestines and bladder, and especially in the action of the iris The habitually dilated pupil depends, Mr Ancell says, upon the diminished power of the iris

> "It has been remarked," he continues, "that it is a better measure of the tone of the muscular system than the touch, since want of exercise alone would relax, for instance, the arm of a delicate female as compared with that of a labourer, but the iris is employed alike by rich and poor Whatever the physiological explanation of the action of the iris, the habitual dilatation of the pupil as it often occurs in tuberculous subjects, is an important sign of the general constitutional debility" (p 47)

It is a fact that an extreme degree of tuberculosis may exist, and yet the intellectual faculties be highly developed All tissues save the nervous may in a given case suffer, whence, then, comes this exemption of the nervous centre from structural change? This question is met by Mr Ancell thus

> "Although the nerves are developed from cells, the dependence of the medullary matter upon the proper material of cell growth appears to cease at a very early period of embryotic existence, and the medullary portion of the nervous structures becomes a secondary deposit in the cell membrane The materials constituting the proper substance of nerves, with all other materials employed in building up and sustaining the animal economy, are equally derived from the blood, but they differ *in toto* from the materials required for the nutrition and growth of cellular structures The nervous tissue differs from other tissues in that it never can, like them, be formed at the expense of the cellular tissues This being the case it by no means follows that a special morbid condition of the blood, which furnishes a morbid blastema to the cellular gelatinous, and fibrous tissues, should necessarily furnish morbid materials for the nutrition of the anatomical nervous system In many of the most unequivocal cases of the tuberculous constitution, the anatomical structure of the nervous system is perfectly intact" (p 19)

An explanation which will not be found very satisfactory by those who are not already of Mr Ancell's opinion

The second chapter is devoted to the signs and symptoms of tuberculosis As Mr Ancell regards the tuberculous predisposition of other writers to differ in degree only from established tuberculosis, he retreads in this chapter the ground he has gone over in his first "The morbid condition of the blood," he writes, "in tuberculosis consists, in an aggravated degree, of many of those deviations from a healthy state enumerated in the preceding chapter" The fact that the fibrine is so very often found to be in excess in this stage of tuberculosis, and the clot small and firm, and covered with a buffy coat, is considered by Mr Ancell to result from the frequency with which local inflammations occur in the progress of tuberculosis "The blood," he says, "has rarely, if ever, been examined in its simply tuberculous state The albumen is *probably* further increased in proportion, and the albumen and fibrine are *in all probability* more vitiated in quality (The italics are our own) The blood-discs certainly continue to decrease in number as the disease advances

The atrophy which is so marked a feature in advanced tuberculosis—

> "Is not produced by a deficiency of blood only—a very common cause of atrophy—nor by fever, nor by excessive evacuations, nor by any defect of primary digestion, for in many cases neither of these circumstances have existed The emaciation of tuberculosis is attributable to a specific vitiation of the blood producing a corresponding vitiation of the blastema, and mal nutrition of cells, the vital power of which being below the healthy standard, there is a consequent excess of the disintegrating over the integrating processes" (p 70)

The atrophy, even in cases of phthisis pulmonalis, is attributed by Mr Ancell directly to the diseased condition of the blood "There can be no doubt," he says, "that it is, from the beginning to the end, symptomatic of the general disease" The anæmia is explained in a similar way

Hæmorrhage from the lungs and other parts, Mr Ancell thinks, is often the result of attenuation of the pulmonary vessels, and occurs frequently before there is any deposit of tubercles—nay, is not unfrequently the primary exciting cause of the local disease On this point, however, it does not appear to us that the evidence is very satisfactory Diminished breathing capacity, and short, dry, hacking cough, may be observed as the result of the blood-disease when no local affection of the lungs is present

A section of this chapter, on Febricula as a result of Tuberculosis, contains some very interesting matter The febrile state which often precedes the deposit of tubercle is, we are satisfied with Mr Ancell, frequently overlooked or misinterpreted, and the practitioner is astonished to see his patient cut off a few days only after his attention has been seriously arrested on the case, and then to find, after death, grey granulation scattered through the lungs or the pia mater This febrile state is thus described by Mr Ancell

> "It consists at first of a general sense of uneasiness, languor, and feebleness, aggravated by the slightest exertion or irregularity of diet There is a slightly increased temperature of the body generally with a tendency to cold, and a great susceptibility to the action of external cold, the skin being frequently warmer, and acrid or arid, except at the extremities which are colder than natural, there is slight thirst, a suppression of natural perspiration, but after accessions of the feverishness, and during sleep the skin becomes pale, soft, perspirable, and cold to the observer There is great susceptibility to cold and tendency to slight rigor, this chilliness in connexion with the feverish heat being a very constant symptom, and of considerable importance in a diagnostic point of view" (p 105)

The breath is heated and fetid, the bowels are irregular, and the digestive organs generally deranged, the urine deposits a whitish sediment, and the patient more or less rapidly loses flesh Sometimes this febrile state at its outset observes "an irregular periodicity," coming on after meals or in the evening or morning, but after a time it becomes continued Mr Ancell points out the danger of confounding this febrile tuberculosis with that 'feverishness of digestion observed after meals in many persons who have in no respect the slightest indication of tuberculosis" With reference to its diagnostic value, Mr Ancell says, "When it occurs simultaneously with, or supervenes on the anæmia, emaciation, and debility, we may be quite sure that the case is verging upon the deposition of tubercle

> "The feverishness of tuberculosis," continues Mr Ancell, "is of more positive value as a diagnostic sign than either of the other symptoms—than the anæmia, the emaciation or the debility No subject of the disease has this symptom in a well marked form, with the chilliness I have mentioned, without being at least on the verge of the deposit of tubercle Since this feverishness also passes by almost imperceptible degrees into hectic fever when it exists we must always entertain the suspicion of a deposit of tubercle" (p 107)

Still, Mr Ancell observes, these general symptoms may occur "without any local deposit whatever"

The third chapter is on Tuberculous Deposits, and includes a consideration of the several modes of origin, seat, physical and microscopic characters, chemical constitution, and phases of metamorphosis of scrofulous pus, of tubercle, and of the black colouring so often found in company with the latter Having in a future article to discuss the subjects considered in this and the succeeding chapter on Special Pathological Anatomy of Tuberculosis, we shall pass them by, merely remarking that they contain most full accounts of the observations of various pathological anatomists on the subjects treated of in them Two hundred pages are devoted to the fourth chapter alone Mr Ancell's materials are drawn chiefly from the writings of Laennec, Cruveilhier, Andral, Carswell, Louis, Hasse, Rokitansky, Rilliet and Barthez, Lebert, Legrand, Clark, Todd, Rainey, Clendinning, and Boyd.

The fifth chapter is a very valuable one; it is on the Causes of Tuberculosis. Mr. Ancell commences this chapter with the apt remark, that "volumes have been written on the causes of those diseases of which tuberculosis is the parent, and very little that is satisfactory can be elicited from them." The causes of the local affections have been strangely commingled with the causes of the general disease. "Sir James Clark made the distinction," he continues, "very clearly, but statistical writers have very little regarded it." The importance of this distinction, practically as well as theoretically considered, must be admitted by all who have had their attention directed to this subject, and we are surprised that writers on the etiology of phthisis should, after it was once distinctly pointed out, have disregarded it.

To attempt an analysis of this chapter would carry us far beyond the limits assigned to this article. Mr. Ancell introduces it thus:

> "On a careful consideration of the whole subject of causation, it appears that the specific morbid condition which constitutes tuberculosis can only result from hereditary transmission by the blood of one or both parents, or be produced by a modification of the blood of the individual. The causes of tuberculosis as we recognise them in practice are accordingly—
>
> "1. The hereditary transmission from parent to offspring of the predisposition to the disease or the disease itself.
> "2. The causes of the production of the disease in the individual.
>
> "I propose to treat of these two totally different subjects in two divisions, but in order that their scope may be somewhat more fully before the reader at the outset, I may here state that the causes which have been assigned of the original production of the disease in the individual, admit also of a twofold division into—
>
> "1. The cause of acquired tuberculosis in the embryo and fœtus.
> "2. The causes of acquired tuberculosis after birth.
>
> "The latter again are divisible into—
>
> "A. The predisposing causes.
> "B. The exciting causes.
>
> "This arrangement differs *in toto* from any that has been employed hitherto by those who have treated on the etiology of tubercular disease." (p. 373.)

With reference to the still-vexed question of the hereditary transmission of tuberculosis, considered as a general disease, and in regard of its local manifestations, Mr. Ancell, after giving the statistical evidence bearing on the subject, (by the way, how could Dr. Walshe's model paper on this subject have escaped Mr. Ancell?) concludes that—

1. Accidental or acquired tuberculosis in the parent may be transmitted to the offspring, either as a predisposition, or as the disease in its active state. At the same time, the frequency of hereditary transmission is probably less than some authors state.
2. The child may inherit tuberculosis from either parent.
3. Tuberculous parents do not necessarily beget tuberculous children.
4. Parents suffering from scrofula—i.e., the early stage of tuberculosis—transmit an analogous condition of blood to their children, but rarely transmit tubercle, whereas those suffering from tubercle may transmit indifferently scrofula or tubercle, or both, to their offspring.

5 A robust mother rarely gives birth to a tuberculous child, even though the father be tuberculous

6 The tuberculous quality of the blood may be latent in the parent at the time it is transmitted to the child

7 The more intense the tuberculosis in the parent, the more likely is the child to suffer

8 The influence of the mother is greater in transmitting tuberculosis than that of the father

9 The parent may recover from a local manifestation of tuberculosis, and yet the blood-disease may not have been eradicated, so that the blood may remain tuberculous to a sufficient extent to transmit the disease to the offspring

10 "The cessation of the hereditary transmission of tuberculosis in a few generations," proves that "however general the tuberculous constitution may be in the human race, it does not constitute a new species"

11 The blood disease may be so slight at birth as to constitute only a proclivity to the disease

12 If hereditary transmission proceeds from one parent, any cause referrible to the other parent by which the constitution of the child is enfeebled, will increase the pernicious consequences of the tuberculous disease

13 As tuberculous disease may be developed without the action of any known anti-hygienic influences in those who are hereditarily disposed to it, it is an error to affirm that the hereditary tendency is a mere pathological susceptibility to an ultimate and indeterminate disease

14 Hereditary tuberculosis developes itself in the form of phthisis at an earlier period of life than the disease does when acquired

15 Hereditary transmission is so much the less to be feared as the appearance of the disease in a family is removed to a distant generation

16 When tuberculosis is transmitted by the blood hereditarily to several members of the same family, the local development may be different in each One may have hydrocephalus, another, white swelling, and a third, phthisis In other cases, however, the tendency to a special local development is transmitted

17 At the time the parent transmits the disease he may appear to be healthy, although subsequently he may die of phthisis

18 The chances of transmission increase with the number of children born, so that it frequently happens that the younger children die before the elder

19 If a tuberculous husband marries two wives, both equally exempt from the disease, the children of the last wife will be more liable to the disease than those of the first

One hundred and seventy pages of his work are given up by Mr Ancell to a consideration of the causes by which tuberculosis may be produced in the individual The result of his researches may be thus summed up

The most efficient causes in inducing tuberculosis are, a vitiated atmosphere, insufficient muscular exercise, and the depressing passions, the next in importance is defective diet There are many other subordinate ancillary causes, as cold, a dense and humid atmosphere, those influences which diminish or retard the hepatic or digestive functions, or lead to congestions of the chylopoietic viscera, whatever exhausts the nervous power and produces debility To these heads most of the so-called exciting causes of phthisis may, Mr Ancell thinks, be referred

The sixth chapter is on the Essential Nature and General Pathology of Tuberculosis In it the question of antagonism is fully entered into, and the various opinions of writers quoted The following observation by Dr Cotton seems to us to be almost absolutely true, judging from the evidence at present before the profession on the subject "Consumption has no antipathy or antagonism to any other disease, beyond that which is common to every morbid condition" The forms and varieties of tuberculosis are disposed of by Mr Ancell in thirty five pages

The last chapter in both the works before us is on the Treatment of Tuberculosis, only, in Dr Cotton's work attention is especially given to the pulmonary development of the general disease In both works this part gives evidence of having been written by men practically well acquainted with the subject on which they were writing, and both chapters deserve attentive consideration

Dr Cotton and Mr Ancell add their voice to the evidence in favour of the value of cod-liver oil Dr Cotton made some experiments to test the comparative value of cod liver oil and other animal oils, and vegetable oils —viz , train, spermaceti, neat's foot, linseed, almond, and olive oils The common train oil was given by Dr Cotton to fifty patients in different stages of phthisis pulmonalis, and notes of the cases were carefully preserved

"Except in ten instances it was not continued longer than a month, for within this period its inferiority to the cod liver oil became too manifest to justify further experiment In these ten cases it was taken—and for a considerable time—with good effect, especially in those which had reached the third stage there was in each an increase of weight, the cough was lessened some of the most urgent symptoms were relieved and the health improved Upon the whole, however, the benefit appeared less than might have been expected from the cod liver oil" (p 276)

The value of the spermacetic oil was also tried on fifty patients suffering from phthisis pulmonalis In four or five of these fifty cases only did the patients increase slightly in weight "In none was the advantage of this remedy either so conspicuous or long continued as that of the train oil ' Neat's-foot oil was given to twenty patients Some gained slightly in weight, "but the majority appeared little, if at all, improved by its employment, so that this oil, like the others, ultimately gave place to its more trustworthy competitor"

"The oils of linseed, almond, and olive, may be included under one head They were given respectively in about thirty cases, in all of which there was either no improvement whatever, or it was so slight as to render it difficult to determine whether or not the oil deserved any of the credit The cough, however, was generally diminished by their influence but neither the patients' appetite nor strength was materially increased, whilst the olive oil occasionally produced a disinclination for food and slight diarrhœa It was singular, indeed, to observe the rapid improvement which often followed their exchange for oleum aselli Upon one occasion, after the linseed oil had been taken for nearly a month with no success, the cod liver oil completely restored the patient's strength, and added to his weight one stone and one pound within six weeks, and in another example, after having prescribed the linseed oil apparently with signal success, the health being improved, and the weight greatly increased, and whilst imagining that at last an instance had occurred of my expectations being realized, I discovered that, the hospital supply having become exhausted the patient, dissatisfied with his improvement, had been taking of his own accord oleum iselli. I had also," Dr Cotton adds, "many similar illustrations in reference both to the almond and olive oils."

Dr Cotton's experiments, then, afforded very different results from those made by Dr Duncan and Mr Nunn[*] they gave almond oil in the place of cod-liver oil in more than 250 cases, and found it of the same value. One of their patients gained two pounds weekly in weight, and another, four pounds, while taking it. Dr Cotton tried the combination of oil and iodine recommended by the same gentlemen, and found that "in many instances it diminished the patients' appetite, and in none did it afford any obvious benefit." Mr Ancell thinks the fact of the Colchester Hospital occupying an elevated site on a dry soil may account for the differences in the results obtained there and elsewhere, in regard of the changes ensuing on the administration of almond oil, while Dr Cotton remarks that Dr Duncan and Mr Nunn gave the almond oil chiefly in scrofulous diseases, and "not to any extent, at least in cases *purely phthisical*." A mixture of sugar with ox-gall was employed by Dr Cotton, at the suggestion of Dr Thomas Chambers. "In no case was it productive of good, and several times it caused diarrhœa."

Naphtha was prescribed in upwards of fifty cases "in different stages of consumption. In many of these cases the patient was made much worse by the drug. The cough was aggravated, and more or less dyspepsia produced, accompanied by a distressing feeling of constriction across the chest, not "the slightest amelioration of the tubercular symptoms being produced." In two instances only did Dr Cotton see any improvement follow its employment, and in both these cases the patients were put upon an improved system of diet at the same time that the naphtha was prescribed, and one of the two was sent into the country.

"Notwithstanding an early prejudice," Dr Cotton says, "in favour of this medicine I am compelled after a fair trial of its effects, to pronounce it of little or no value in the treatment of phthisis." (p 283.)

Dr Cotton's essay will be found useful to the student and young practitioner, as a guide in the diagnosis and treatment, especially, of cases of phthisis pulmonalis. Mr Ancell's work is remarkable for the care with which he has collected the facts and opinions of other writers on the subject of tuberculosis, for the fairness and clearness with which he has stated those facts and opinions, for the skill he has displayed in arranging his heterogeneous materials so as to construct a systematic treatise embracing the whole subject of tubercle, and in bringing all to bear in support of his theory of tuberculosis and tubercular deposits. Mr Ancell's own opinions are put forward as by a man thoroughly satisfied of their truth, of their importance, and of their practical bearing, and yet the opinions of others are always treated by him with due respect. His treatise on tuberculosis ought to be in the library of every practitioner of medicine.

W Jenner

---

[*] Provincial Medical and Surgical Journal, March, 1850

PART SECOND

Bibliographical Record

ART I —*The Transactions of the American Medical Association* (Instituted 1847) Vol V —*Philadelphia*, 1852 pp 939

THE American Medical Association is a society which may be compared to our own Provincial Association In their volume of 'Transactions' are published the proceedings of the society, and various papers contributed by the members Among the latter we notice a valuable paper by Dr Austin Flint, 'On the Variations of Pitch in Percussion and Respiratory Sound, and their application to Physical Diagnosis,' which we shall take an opportunity of considering at length Dr Dickson communicates a paper on the blending of types in fever, which does not appear to us to be likely to lessen the confusion already existing on this subject Dr Adams reports, in the name of a committee, on the action of water on lead pipes, and on the diseases arising from lead thus introduced into the system Some very interesting cases of lead poisoning are recorded The committee deny the various statements which have been made, respecting the protection given to lead by a coating of salts They allude especially to the "Edinburgh doctrine," as taught by Christison, to the "Boston doctrine" (that the lead is protected by a deposit of suboxide), and to the "London doctrine," promulgated by Hoffmann, Graham, and Miller, that the carbonate of lead is deposited, and prevents the further action of the water They also state, that in the Croton water, which supplies New York, and which is drawn from leaden hydrants, put down ten years since, lead can be detected

We shall take occasion to refer to this important paper when the subject of lead poisoning again comes under our notice Mr Hayward reports on the permanent cure of reducible hernia, and Dr Pope has a paper on the topical uses of water in surgery The whole of the remaining part of the volume, consisting of nearly 700 pages, is occupied with reports on the epidemics of New England and New York, and on the medical botany of the United States These are also extremely valuable papers

---

ART II —*Anatomical and Physiological Bibliography, from* 1849 *to* 1852 *inclusive* By G E DAY, M D, Professor of Medicine in the University of St Andrews (From 'Goodsir's Annals of Anatomy and Physiology)

THIS is a very complete bibliography of all anatomical and physiological papers published in periodicals and 'Transactions' from 1849 to the end of 1852 The names are arranged in alphabetical order We have looked through some foreign journals, and compared them with the 'Bibliography,' without at present detecting a single error

We would suggest, however, that in future bibliographies (and we hope this is not to be the only one), it would be well to have a list, not only of names, but of things, in alphabetical order Reference could then be made with great ease, whereas at present we can learn at once what has been done by any particular *person*, but we cannot learn without great trouble what has been done in any particular *subject*

We hope that this 'Bibliography' will meet with a good reception It deserves to be encouraged, and a proper sale would enable Dr Day to make future numbers more useful still for reference

---

ART III —*Lectures on Surgical Pathology, delivered at the Royal College of Surgeons* By JAMES PAGET, F R S, lately Professor of Anatomy and Physiology to the College —*London*, 1853 Vols I & II

MR PAGET'S lectures on inflammation and atrophy have been virtually so long before the public, and have been reviewed by us at such length on various occasions, that scarcely anything now remains for us to do, as regards the greater number of the lectures, but to express our pleasure at meeting them in their present shape They will now form one of the standard works of English Medicine, and will become a landmark, when the flood of progress has concealed from view less lofty and stable productions The large audiences who assembled in the theatre of the College of Surgeons, to hear Mr Paget's eloquent lectures, will derive scarcely less pleasure from reading them The style is so easy, the illustrations are so apposite and so graphic, and the expression of facts is so pointed, that it is impossible not to see that Mr Paget possesses, in an unusual degree, not only the art of observation, but the still more uncommon one of exposition

The second volume demands from us more than this passing notice It treats of tumours, and is in many parts a new work We shall take an early opportunity of carefully reviewing it, and in the meantime will merely say, that although we differ from it on some points, it is equally distinguished by the two great qualities of range and clearness of thought

---

ART IV —*Researches on the Primary Stages of Histogenesis and Histolysis* By ROBERT D LYONS, M B, &c (From the 'Proceedings of the Royal Irish Academy' Vol V, Part III)—*Dublin*, 1853 pp 16

THIS is an important paper, and will be read with interest in connexion with the elaborate review on the cell-theory, which we have given in a former page Dr Lyons has fully recognised the inadequacy of the cell-theory of Schwann to explain *per se* the formation of organic structure From the examination of the yelk of the egg, of the germinal membrane, and of the plasma poured out on a fresh wound, and from a consideration of the experiments of Ascherson, Panum, and Melsens, he has satisfied himself that "structures can originate under conditions when we cannot suppose any vital organic influence to be present, but when such forces as attraction, cohesion, fusion, endosmose, and exosmose, and the mutual reaction of elements differing in physical and chemical characters, are in full operation" (p 7)

The structures thus formed under simply physical or chemical conditions are as follow organic granules, isolated or aggregated, and then forming granular corpuscles, granular stroma, hyaline membrane, and hyaline vesicles These elements are considered to be, by Dr Lyons, the "rubble-work of the organic edifice," but to be in themselves incapable of generating the higher tissues which occur in the animal frame These higher tissues, originating in a mode of which we have no very clear conception, are plastic corpuscles, fibres, cells, and definite tissues

We are inclined to think that Dr Lyons might have advantageously developed this portion of his subject a little more, as it is one of immense importance in minute anatomy We are convinced that the cells forming in albuminous fluids from physical conditions merely, and to which the writer of this notice has been accustomed to apply the term "corpuscular albumen," are frequently referred to, as evidence of the last remains of life and vitality in a plasma or exudation, as the proof, in fact, of the dying and ineffectual effort of the organism to form a tissue

In the second part of his paper, Dr Lyons enters on a consideration of the changes which the developed tissues undergo in their progress towards decomposition—that is to say, in their passage from complex into simple forms For this process he proposes the convenient term "Histolysis," as an antithesis to Histogenesis To trace the histolytic changes in the animal tissues is a department of physiology or pathology of the highest interest, but unfortunately of no less difficulty, and very little is known of the microscopic changes which a tissue—as muscular fibre, for example—undergoes in its progress from its perfect to its effete condition, when, after having played its part, it vanishes from the body Dr Lyons narrates briefly the microscopic changes in integument, subcutaneous structure, blood, and muscle, decaying when removed from the body He sums up his results thus

> "In considering the chief results arrived at in the study of the process of putrefaction, I am led to believe—
>
> '1st That, concurrently with the first order of chemical changes, a certain order of morphic changes takes place before the final dissolution of organic structures, by the action of chemical and physical forces
>
> "2nd That this series of changes may, under normal conditions, take place very slowly, so that, at the end of many months, and probably of even much longer periods, we are still enabled by the microscope to recognise and identify structures of great delicacy, such as elementary muscular fibre, and that this knowledge admits of important applications
>
> "3rd That in this process of histolysis the first changes consist in the softening, disunion, and separation from each other of the morphic constituents of the tissues, each of which is then subjected to a process of disintegration
>
> "4th That granules and granular corpuscles appear at an early period, arising probably from recombinations of the particles of the organic fluids Animalcules appear at this stage
>
> "5th That granules, corpuscles, vesicles, cells, and granular masses of various kinds and sizes, may form in fluids and tissues undergoing histolysis, in which no such elements exist when in their normal states

"6th That generally in the progress of histolysis, structures very similar to those which are arranged under the first group, or the aplastic elements of histogenesis, form at different stages, and that they exhibit the same modes of growth and development, but, like them, are incapable of producing higher forms

"7th That these morphic elements of histolysis pass gradually into lower forms, exhibiting occasional instances of endogenous fissiparition, granular disintegration, and other changes, and that the cellular and corpuscular elements, by forming media for endosmose and imbibition, may aid in the disintegration of contiguous structures

"8th That certain elements may pass directly into a state of molecular disintegration

"9th That certain corpuscles of peculiar characters, and not identical with any known normal elements, are occasionally formed

"10th That a period arrives at which chemico physical forces prevail, which is evidenced by the passage of certain elements into crystalline forms, others passing off by volatilization, solution, &c, and that in this way the final dissolution of a tissue is accomplished, the several morphic changes which take place probably facilitating and preparing the way for the action of chemical forces '

In addition to this, we need only notice, that Dr Lyons has observed blood-crystals to form in one case in twenty six hours, and in another (the blood of the duck) to be present two years after removal

We trust Dr Lyons will continue his important researches

ART V—*The Harmonies of Physical Science, in Relation to the Higher Sentiments, with Observations on the Study of Medical Science, and on the Moral and Scientific Relations of Medical Life* By WILLIAM HINDS, M D—*London*, 1853 8vo, pp 195

THE latter half of the title of this book expresses the nature of its contents better than the first half It is a treatise on medical life in its moral relations, with incidental discussions on the physical sciences as bearing especially on moral and physical training We have read it with great pleasure it is written very clearly and agreeably, and bears in every page evidences of thought, without anything very striking or original We should say that the author commenced his work without a definite idea of its scope and limits, but being urged on merely by the knowledge, that he had thought over and had something to say on a good many closely-connected subjects In some parts it is like Paley, in others like Percival, now we seem to be reading a chapter on the evidences of design, and now we turn to a discourse on medical ethics We do not quarrel, however, with this discursiveness, the book is not, perhaps, the less interesting on that account

In the last chapter we find some very useful remarks on the "Profession of Medicine and its Relation to the Community" Dr Hinds points out clearly the manifold influences which bear on the medical practitioner, and which act with such different effect on variously constituted minds After mentioning the practical and scientific elements which, in just proportion, should constitute the basis of the practitioner's professional character, Dr Hinds continues

"On this foundation there must yet be a superstructure of qualities and qualifications of a different kind, and this brings us to a brief consideration of the *motives* which should influence a man in adopting and pursuing the profession of physic as a calling. If these motives were confined to the mere choice of a profession, at the commencement of life, their influence would be of less value, inasmuch as the choice is often made either by proxy, or before the agent is thoroughly capable of taking enlarged views, in a moral and professional sense. This, however, is not the case. The motives and springs of conduct which should actuate the practitioner of physic affect the whole of his career, and must be kept before his mind as a great and enduring principle of action. In their nature, these motives must not be sordid. The man who pursues his profession with no higher considerations than those which arise out of a love of wealth, will not only often fail to accomplish his aims, but will almost inevitably degrade the high and noble calling in which he is engaged, and bring dishonour upon a profession *the proper pursuit of which is truly calculated to ennoble the sentiments and to raise the relation between practitioner and patient to something higher than a mere sordid or business relation.* It is a truth to which society has given an unreserved adhesion, that a practitioner wanting in humanity and not self-sacrificing is not in his proper sphere. A pure and elevated humanity, and self-denial, with a love of knowledge, are amongst the great aims which should be found in the bosom of every man who undertakes the duties and responsibilities of the practitioner of physic. Without this moral heroism as a mainspring of his conduct, he will not ennoble his calling, nor be at peace with its requirements." (pp. 159—161.)

This is perfectly true, and we will venture to say that no profession can offer more numerous instances of men constantly acting under the influence of these noble sentiments, than our own. But there is another side to the question; if we have our duties, the public have theirs; our exertions have their value, to claim which is not avarice, but justice. Let us quote again from Dr. Hinds a few pages further on.

"In the career of practice, great forbearance is often needed on the part of medical men, and frequently nothing but an appeal to high principle will enable them to bear with composure the injustice which both individuals, and sometimes even bodies, will inflict upon them. It will accord with the experience of nearly every medical man to have been habitually subject at some part of his career, to acts of injustice the most flagrant from individuals who have sought his professional aid, and indeed the mass of our profession, at all points of their professional career, are more or less visited by the same reckless want of thought and injustice. While the community in general hold it cruel and shocking for a medical man to exercise a discretion when he is called to any case supposed to require immediate aid, and while universally medical men themselves freely and fully recognise the claims of the suffering, there are multitudes of persons to be found above the classes of the poor who will resort to shifts the most dishonourable to avoid remunerating the medical attendant for his services. Not a word could be said if these cases were confined to the really poor, whose poverty is well known to be, with nearly all medical men, a sufficient justification for them to seek such services without incurring responsibility. Unfortunately this is far from being the case. Persons in respectable positions and with ample means will practise the same injustice. The law courts alone will testify to the fact, and perhaps not a ten thousandth part of such cases ever gets into these courts. Moral responsibilities for medical services of no ordinary kind are for ever being sought to be got rid of *in toto* by some amongst all classes of the community, on mere technical grounds. A gentleman has a servant attacked with apoplexy or epilepsy, and in the alarm and excitement, orders the nearest medical man to be summoned. The servant, after a recovery from a long illness perhaps, removes to a distant part of the country, and the gentleman, thinking the servant responsible, allows the medical man to remain unpaid. A traveller on a railway meets with an accident, and has a limb fractured, and is carried insensible to a place of rest, while the railway

> official, or any one with sufficient humanity, seeks the nearest medical man, who leaves his home and bed, perhaps in the middle of the night to perform an operation The traveller recovers, and leaves the medical man, who was called in by the railway official, to appeal to the law to ascertain who is responsible for payment Thousands of instances, in great variety, and of frequent occurrence, might be enumerated, of injustice to medical men, more or less resembling these
>
> The too frequent perpetration of acts of heartless injustice with which he meets in his career appeal strongly to his conservative views of self, and to the duties he owes to his personal interest, and tempt him with the necessity to relieve suffering and save life only where there is a distinct comprehension of responsibility for his remuneration Heaven forbid that any medical man should be ever found to yield to the temptation and carry this inhuman principle into practice That he does not, however, is not attributable to the generosity or justice with which he often meets during his professional experience" (pp 163—166)

We are not quite sure that we thoroughly comprehend how Dr Hinds would solve this somewhat knotty point No one could wish our profession to lose its liberal and benevolent character, in a too earnest desire to obtain an equivalent for its services, on the other hand, there cannot be a doubt that great harm has resulted from a too lavish charity on its part It appears to us that the proper mode of viewing the case is exactly the same as that in which almsgiving is now universally considered The medical practitioner, after giving his advice, is entitled to receive an equivalent, if he gives up this equivalent he is bestowing an alms on the receiver, and it is as much his duty to determine that the receiver is a proper person to receive alms, as if he gave money, instead of money's worth He is guilty of indiscriminate almsgiving, and encourages exactly the same evils as the man who maintains a corps of idle beggars in the streets of London, if he is not quite clear on this point He does, however, more harm even than this, as he injures also his brother practitioners and the whole profession That which is easily got is seldom highly esteemed and in addition, the public has been so accustomed to disregard remuneration to medical men, that absolutely a man who is merely contending for the rights most justly his, is looked upon by too many in the present day as illiberal and money-seeking We must blame ourselves if the public are slow to perceive what are *our* just rights, and *their* proper duties We admit that there may be a difficulty in knowing always who are the poor, and then we may safely err on the side of charity, but every practitioner must know that there are hundreds of cases in which he has given gratuitous services to persons who ought to have returned an equivalent

We quite agree with Dr Hinds, that in a case of emergency a medical practitioner should not think of assuring himself, first of all, that his remuneration is secure His first duty is clearly towards the patient, whose life may be in danger, but this immediate crisis being past, we are at a loss to know why he should not then attend 'to the duties he owes to his personal interest" In so doing, he cannot lay himself open to the charge of avariciousness or sordidness, he is performing an act of simple justice, both towards himself and towards those who are practising the same calling

At a subsequent page, Dr Hinds has some pertinent remarks on the behaviour of the members of the profession towards each other, with a quotation from which we conclude our notice of this pleasing work

“If a Christian liberality and justice be imperative as a rule of conduct between the medical man and his patients and the community, it is equally imperative amongst his fellow practitioners. Perhaps nothing produces such lamentable impressions upon the community, in reference to the medical body, as a selfish rivalry and ill nature amongst themselves.

“Feuds, and selfish jealousies, and vindictive rivalries, amongst distinguished members of our profession, are injurious to our moral and social status, if they do not even outrage the proprieties and laws of polite society. They degrade us below a first class moral and social rank, and lower our standard, even in the eyes of the less refined and less cultivated portion of the community. It is a truth which we shall all readily acknowledge, that every feeling opposed to a frank and genial intercourse and entire good faith amongst the members of our body, is totally unworthy of us as a profession. It is not to be denied that this high standard in our mutual ethics can be attained and preserved only by means often of the most firm and determined forbearance. Through the inadvertence or whims or misrepresentations of patients, medical men are not seldom brought painfully into opposition or collision with each other. In all such cases the only complete safeguard is a reliance upon the honour of each other, and a firm determination to allow no circumstance, however irksome and trying, to induce a departure from a true and gentlemanly forbearance. Every member of our body, before he can assure himself of his perfect fitness for this task of excellence, must be prepared to make sacrifices of self-interest; for though a mere sordid personal interest is not the most prevalent of the causes of ill feeling, and the other allied evils complained of in our profession, yet sacrifices of personal interest are occasionally nevertheless demanded.” (pp. 187—190.)

Art. VI.—*Commentaries on the Surgery of the War in Portugal, Spain, France, and the Netherlands, from the Battle of Rolica in 1808, to that of Waterloo in 1815.* By G. J. Guthrie, F.R.S. Fifth Edition, revised to 1853. pp. 603.

Although a fifth edition, this book may justly claim something more than a mere announcement. We shall therefore review it in a future number, and shall now merely state that this edition is much improved; it is full of details and illustrative cases, and the commentaries upon them have been carefully re-considered. Many of the cases are of a most extraordinary kind, and illustrate the wonderful recoveries which sometimes occur after apparently the most desperate wounds. They are related in a very vivid manner; and in reading them, we have had constant occasion to admire the way in which the prominent points of the case are brought out. We defer our criticism on the surgical doctrines till a future occasion.

Art. VII.—*On the Nature and Treatment of the Deformities of the Human Frame: being a course of Lectures delivered at the Royal Orthopædic Hospital, in 1843. With numerous Notes and Additions to the present time.* By W. J. Little, M.D.—*London*, 1853. pp. 412.

Dr. Little's treatise on the ‘Nature and Treatment of Clubfoot’ has received much attention from the profession, and has decidedly been the means of making widely known some great surgical improvements. The present work contains the substance of the former one, and much original matter, and is certainly the most complete treatise we possess on the subject of deformities The descriptions of the distortions are clearly given, and are illustrated by very good woodcuts The only omission we notice in it is, that there is no account of the operation lately proposed by Mayer for knock-knee* (*genu valgum*) In speaking of lateral curvature, Dr Little enumerates the various means adopted for its removal under eleven heads—viz

1 Improvement of the general health
2 Mechanical supports, or scaffoldings
3 ——— compression of lapsed parts
4 ——— elongation of the whole trunk
5 Surgical division of contracted muscles
6 Recumbency
7 Gymnastics
8 Automatic exercise of particular muscles
9 ——— rectification of equilibrium of the column
10 Topical application of medicines
11 Electricity

The remarks made under each head are judicious Mechanical supports are neither too much discountenanced, nor too much lauded The effect of gymnastics, however, is too shortly discussed, and the "Swedish exercises' are too decidedly condemned We cannot too strongly discountenance the quackery which has been, and is, associated with these exercises, but that some of the practice might be properly incorporated into our mode of treatment of spinal curvatures, we cannot doubt

We recommend Dr Little's book as a most useful one, every page bears marks of the good sense of the writer, and of his profound acquaintance with the subject

---

ART VIII—*On the Use of an Artificial Membrana Tympani in cases of Deafness, dependent upon Perforation of the Natural Organ, to which is added a paper, entitled, Ought the Tonsils to be Excised in the Treatment of Deafness?* By JOSEPH TOYNBEE, F R S—*London*, 1853 pp 46

THE first of these two papers is divided into the following sections

1 On the Structure of the Healthy Membrana Tympani
2 On the Functions of the Membrana Tympani, Tympanum, and Eustachian Tube
3 On the Formation and Use of an Artificial Membrana Tympani

The third section alone requires any notice The artificial membrana tympani is made of a thin slip of gutta percha or vulcanized india rubber, placed between two thin and small plates of silver, or between two delicate silver rings, smaller in diameter than the gutta percha At the time of application, more or less of the excess of gutta percha may be cut off The artificial membrane is then moistened and passed down to the remains of the membrana tympani, it cannot pass beyond into the cavity of the tympanum without causing pain, which should be the signal for its withdrawal. It can be withdrawn from the meatus by means of a silver wire attached to one of the plates. The cases in which this apparatus has been found useful, appear to be similar to those which are temporarily so much improved by the use of moist cotton—viz., perforation of the membrane from otitis after scarlet fever, measles, &c., or idiopathic inflammation and ulceration of the membrane. Nine satisfactory cases are related in proof of the efficacy of this method.

---

* Die Osteotomie als neues Orthopædisches Operations verfahren, von Mayer (Wurzburg Verhandl vol ii, p 224, and vol iii)

The second paper is a protest against the practice of excising enlarged tonsils for the cure of deafness.

ART. IX.—*Habit, Physiologically considered.* (*A Lecture delivered at the Bristol Literary and Philosophical Institution.*) By JOHN ADDINGTON SYMONDS, M.D. (Reprinted from the 'Psychological Journal,' for July, 1853.)—*London*, 1853. pp. 28.

THIS is a very clever lecture on a subject of which it must have been somewhat difficult to treat before a mixed audience.

The first fourteen or fifteen pages are taken up with illustrations and descriptions; it is not till the seventeenth page that we come to the all-important question—Why should mere repetition dispense with volitional exertion?

"To answer this fully," says Dr. Symonds, "would require a long and abstruse discussion. I must content myself with a summary statement of what seems to be the explanation of the fact."

This explanation is, that

> "In volitional movements, the action of the commissures between the sensational and their related motor centres is excited in the first instance by the will. But the repetition of more or fewer of these incitements appears to suffice for the growth and development of the commissure to a degree of strength and activity adequate to the performance of its functions independently of the will."—(p. 17.)

This strikes us as a very improbable hypothesis.

In the latter part of the lecture Dr. Symonds slightly touches on what he terms the automatic action of the brain in intellectual processes, the self-evolution of thought, what has been termed by Dr. Carpenter, "unconscious cerebration." This is a most curious and interesting subject, scarcely yet explored, and likely to lead to some modification of psychological doctrines. We would suggest to the directors of the Bristol Literary Institution, that they should request Dr. Symonds to enter on this subject in a second lecture; it will, we are convinced, amply repay them.

ART. X.—*Remarks on Hysteria, in connexion with Hydrophobia and other Convulsive Affections.* By JOHN DALZIEL, M.D.—*Edinburgh*, 1853. pp. 32.

THIS paper was written by the author more than twenty years ago, and was sent for publication to the 'Glasgow Medical Journal,' in which an abstract of it was published in 1830. The author has now published it *in extenso*, in order to show that his views on hysteria and hydrophobia correspond with those lately promulgated by Dr. Marshall Hall. The following quotation expresses his opinions:

> "1st. The *globus hystericus*, as well as the similar affection of the throat in hydrophobia, occasioned by the idea &c. of liquids, is a spasmodic stricture of the muscles of the throat (*glottis*), whereby respiration is obstructed.
>
> "2nd. *Obstructed respiration*, whether *suspended* or *impeded*, occasions cerebral congestion, as well as that feeling of *general uneasiness* designated *sensation of suffocation*, which attends the paroxysmal exacerbation in both the diseases under consideration.
>
> "3rd. Cerebral congestion and the sensation of suffocation, separately or conjointly, may, especially in an irritable habit, occasion *convulsion*." (p. 6.)

With respect to hydrophobia, we may quote one or two passages which explain clearly enough the author's meaning:

> "It is my express object to show that the convulsion is to be attributed entirely, though indirectly, to *obstructed respiration*, and not to the accompanying circumstance of the local spasm, or pain attendant on the spasm. The competency of obstructed respiration to occasion convulsion will be fully made out by establishing the second and third propositions; but the second term of the second proposition, and the first of the third, are also admitted. The only link wanting then to complete the chain, is the proof that obstructed respiration produces cerebral congestion." (p. 8.)
>
> "If a cure for so horrible a malady is ever to be discovered, there is a rational ground for hope that the operation of *bronchotomy* will at least make one step of approximation towards it; and it will at all events afford the disease ample opportunity of fully developing itself in its undisguised essential character, and place the patient in circumstances more favourable for the salutary operation of medicine." (p. 29.)

Certainly the similarity between these views and those of Dr. Marshall Hall is singular. How far they are correct we shall not stop now to inquire.

ART. XI.—*Reports by* NEIL ARNOTT, M.D., *and* THOMAS PAGE, Esq., C.E., *on an Inquiry ordered by the Secretary of State relative to the Prevalence of Disease at Croydon, and to the Plan of Sewerage.* (Parliamentary Paper.) 1853.

IN both a medical and a social light, the fever which prevailed at Croydon after the new plan of sewerage had come into operation, is deserving of very careful study. As we have reason to believe that other medical statements will yet appear, we shall not now consider the very able paper before us at any length. Both Dr. Arnott's and Mr. Page's reports are excellently done, the former, treating chiefly the medical and sanitary problems presented during this outbreak, the latter, the engineering operations.

The disease at Croydon was, we are informed, in the main, well-marked "typhoid fever," this term being used in the sense in which it is employed by Louis and Jenner, but other anomalous forms of disease presented themselves, as in the case of Dr. Chalmers (Report, p. 8), such as low erysipelatous inflammation of the mouth and fauces. Although exact records are yet wanting, we believe that one of the most interesting problems in ætiology will be solved by this epidemic, and that the arguments to show that the effluvia from drains can produce a specific fever, capable, subsequently, of propagation from person to person, by contagion, will be made complete On this point Dr Arnott has some illustrative cases in his interesting general remarks on the origin of fevers, published at the conclusion of his report

Mr Page's report is, of course, an engineering statement, but is made with such clearness and precision, that there is no difficulty in following it throughout, and in giving assent to all the conclusions

Mr Page adds another able protest against the engineering mania of the General Board of Health, a mania which has already been unequivocally condemned by the united voice of the engineers, as expressed in the discussion at the Institute on the small-pipe drainage But he also shows that the peculiar difficulties incident to the drainage at Croydon were quite unprovided for, and that the General Board of Health made everything worse by a number of ill-advised directions

ART XII —*Small Pox and Vaccination Copy of Letter from Dr Edward Seaton to Viscount Palmerston, with inclosed copy of a Report on the state of Small Pox and Vaccination in England and Wales and other countries, and on Compulsory Vaccination, with Tables and Appendices, presented to the President and Council of the Epidemiological Society by the Small Pox and Vaccination Committee* (Parliamentary Paper, 3rd May, 1853)

ALTHOUGH we do not propose at present to enter on a critical review of the subject of vaccination, we are unwilling to omit the earliest opportunity of expressing our admiration of this admirable Report The Small Pox and Vaccination Committee of the Epidemiological Society consisted of Messrs Grainger, Marson Hunt, Waller A Lewis Kesteven, Aikin, and Seaton, the latter gentleman being the honorary secretary These gentlemen put themselves into communication with the several official bodies, and with numerous private practitioners in this country, and, by means of the British ambassadors, with the various institutions on the Continent The result is a most complete account of the prevalence and mortality of small pox in different countries, and of the means taken to guard against it through vaccination The general result of the investigation is, that the protective influence of vaccination is affirmed, and that the virtual obliteration of small pox is proved to be a matter of easy accomplishment for those who will adopt the means of doing so which Jenner pointed out These conclusions are deductions from the largest mass of statistical evidence which has ever been brought to bear on the question The Registrar general's returns, the documents of the Poor-law Board, official statements from Mr Wilde for Ireland, from Dr Stark for Scotland, and from more than twenty foreign governments, form the staple of the Report It would be extremely difficult for us to present an abstract of the tables, but the following statement will give their main features

1 To prove the influence of vaccination in England

Out of every 1000 deaths in the half century from 1750 to 1800, there were of small pox	96
Out of every 1000 deaths in the half century from 1800 to 1850, there were of small pox	35

Either small pox has become naturally milder, or is treated more successfully, or the mortality from it has been diminished by vaccination That the last is the true cause can be proved from independent evidence

2 To prove the influence of vaccination on the Continent

In various German states, sufficient evidence can be obtained to show that, before vaccination was used, out of every 1000 deaths, there occurred from small pox	66 5
After vaccination there occurred	7 26

3 To prove that in countries where vaccination is most perfectly carried out, small pox is least mortal

(*a*) In this country, where vaccination is voluntary, and frequently neglected—

	Deaths from Small Pox	Deaths from all causes
London	16	1000
Birmingham	16 6	,,
Leeds	17 5	,,
England and Wales	21 9	,,
Paisley	18	,,
Edinburgh	19 4	,,
Perth	25	,,
Glasgow	36	,,
Dublin	25 66	,,
Galway	35	,,
Limerick	41	,,
Connaught	60	,,
All Ireland	49	,,

(*b*) In other countries where vaccination is more or less compulsory—

	Deaths from Small Pox	Deaths from all causes
Westphalia	6	1000
Saxony	8 33	,,
Rhenish Provinces	3 75	,,
Pomerania	5 25	,,
Lower Austria	6	,,
Bohemia	2	,,
Lombardy	2	,,
Venice	2 2	,,
Sweden	2 7	,,
Bavaria	4	,,

That the immense difference apparent in these two tables is rightly attributed to vaccination, there can be no question

The Report enters at some length into the comparative use or neglect of vaccination in various parts of England, and shows, as might be expected, that in many places the majority of the population are altogether unprotected

These statements lead, of course, to the conclusion that the only proper course is to make vaccination compulsory Whatever may be the defects of Lord Lyttelton's bill in some respects, it is undoubtedly one of the most important measures of the session We are at a loss to know why it has met with so much opposition Its defects will be easily amended, and if they were twenty-fold, it must be better to put up with them, rather than delay for another year the benefits of this wise measure

PART THIRD

Original Communications

ART I

The Blood—its Chemistry, Physiology, and Pathology By THOMAS WILLIAMS, M D Lond , Extra Licentiate of the Royal College of Physicians , formerly Demonstrator on Structural Anatomy at Guy's Hospital, and now of Swansea

THE fluids and the solids in all biochemical inquiries should rightly be studied in conjunction They interblend, substantially and dynamically, with such intimacy, that to indicate a partitional boundary were to divorce what nature has united They reciprocally cede and accept They mutually act and react Fluidity and solidity are the first conditions of all organized matter The aeriform state has nowhere a single illustration Every gas must become a fluid as the fundamental term of union with the elements of the living body The fluid is the active condition, the solid, the passive The latter is sedentary, the former, locomotive The former is the scene of rapid molecular transformations, the latter is distinguished for its stability between these two extremes there exist manifold intermediate conditions In the category of the nutritive fluids, unmixed fluidity is not known solids are introduced Fluids are not self-productive, they require either the direct or the catalytic agency of solids The fluids even of the vegetable organism bear floating corpuscles In the vegetable economy, it has hitherto been falsely supposed that the cells of the fixed tissues traversed by the fluids effect that change which, in the instance of the animal fluids, is accomplished by the self borne corpuscles The formative capacity of a fluid, in the abstract, is limited to a low standard Albumen cannot mount into the condition of fibrine in virtue of any self-originated and self-sustained inherent molecular activity, the agency of a third body must be interposed This is the "doctrine of the schools"

The blood is a wondrous epitome of fluids and solids In complexity of composition it has no parallel He who would resolve it into its components, must in himself unite the qualifications of the chemist and the microscopist, the anatomist and the physiologist The chemistry of organized beings has now assumed the exalted rank of a rich and varied science The microscope, at first a costly toy, has grown into its inseparable handmaid Micro chemistry is opening upon the mind like a vast panorama, opulent in wonders and boundless in limits The microscope conducts the eye to the confines only of visible form, the formless eludes its scrutiny it is, emphatically, an instrument fitted to define the geometric properties of bodies Into the world of fluids it cannot penetrate Fluidity is a condition of matter which it cannot appreciate. Of solids, it takes cognizance only of the state of quiescence; it does not reveal the "forces" impelling them to activity; it is an apparatus of inquiry into the statics, not the dynamics, of matter. In micro-chemical inquiries, its peculiar value consists in empowering the eye to read and register the changes of form and colour wrought by test fluids upon solids: the knotted tangle of a multiplex compound is thus gracefully unwound; it is in this elegant manner that the mind arrives at a knowledge of the chemical constitution of bodies—even those so minute in mathematical dimensions as to occupy the remote frontier-line which divides the fluid from the solid state, the extreme verge of morphological substance.

From the arena of zoochemical research, in these unsuperstitious days, all substanceless divinities must be exterminated. The era of the *anima mundi* has become historic. The "essence of life," *vis vitæ, vital principle* (Prichard), *nisus formativus* (Blumenbach), *organic force* (Muller), *organic agent* (Prout), *vis medicatrix naturæ* (Hofman and Cullen), now number amid the effete jargon of an extinct nomenclature. The "illuminating orb" of a new mental life has arisen above the edge of the eastern horizon. "Abstract terms" are as luring as "golden calves;" they demand the homage of believers. The "*principle* of motion," the "*principle* of gravitation," the "*principle* of attraction," are the relics of an entombed terminology. The "*life*" of the blood is an ontological expression which belongs to a former phase of science. No *divina particula auræ* swims in this fluid. Modern philosophy displays a tendency to refer all phenomena to the *laws* of matter—*directions* of action first implanted by the Deity. A "law" in its ontological sense is a *deus ex machinâ*; it sounds as though endowed with an existence distinct from the body—as something superadded to the organism—like magnetism to iron, heat and light to a luminous body (*sic*). The word "*principle*" is characteristic of a less advanced state of science; it should *now* be used only as the final letters of the alphabet are employed by the algebraist—to denote an unknown element, which, when thus indicated, is more conveniently analyzed. It is customary, even now, to speak of the "principle"—the agent, of heat, light, electricity, magnetism, gravitation, &c.—as signalizing severally the undetermined causes of familiar phenomena. When these phenomena, in the progress of science, fall under a more just interpretation, they come to be referred to the primary properties of matter; they then may be deduced by demonstrative reasoning, like geometric theorems from the postulates on which they are founded. But in the science of physiology the term "law" has been employed in a less justifiable acceptation; a "principle," a "force" has been personified, and invested with a spontaneity of action. As an expression descriptive of the "conditions," the "circumstances" under which the actions and reactions of the material elements of the organism originate and proceed, it may be tolerated as a convenient phrase; the physiologist, however, idolatrously animates it with a creative and directive power, in virtue of which it acts upon matter, removing its particles from the pale of physical and chemical laws, transforming them into organized tissue, endowing these tissues with novel properties, prompting them to action, opposing resistance to injurious influences, defining the cessation of these acts of the organic body as synonymous with the departure of the "vital principle" A "law" ought not to be defined as altogether resident in the mind of the observer The mind only links certain acts into a certain order of occurrence, this *order of occurrence* is coeval with matter, it moves, "displays its forces," "exerts its energy," only in a given direction This *mode* of occurrence is the *law,* it is the final impress of the will of Him who made matter, it is an ultimate fact beyond which philosophy cannot penetrate The task of the organic chemist, then, lies not in a search after an *ignis fatuus,* a *vital principle,* he is required only to define the terms of events, the conditions of phenomena, as they occur in the living organism *Words* must be used to indicate events The exigencies of language will oblige the use of such expressions as vital force, chemical force, affinity, property, &c It will, however, be now understood within what latitude such expressions are to be limited

Two new ideas have recently taken birth in science The mutual convertibility of the "forces' is an accepted doctrine, and the transmutability of matter (Dumas, Faraday) is no longer held as an alchemic extravaganza The facts of allotropism, established by the genius of Schonbein, and the late prophetic speculations of M Dumas* on the chemical, electrical, and mathematical *progression* traceable through the properties of isomeric or conformable bodies, awaken in the mind of the modern chemist a reverence for the disentombed manes of alchemy The transmutable bodies group themselves in nature in triads, or ternary series thus chlorine, bromine, and iodine, sulphur, selenium, and tellurium, calcium, strontium and barium, lithium sodium, and potassium The members of these triads, severally, are capable of replacing one another in chemical compounds "When three bodies having qualities precisely similar, though not identical, are arranged in succession of their chemical powers, there will be also a successive arrangement of mathematical powers, indicated by the respective atomic numbers of the substances, and amenable to every mathematical law" "That this symmetry of chemical with mathematical function points to the possibility of 'transmutation' is unquestionable, yet not transmutation in the sense of the old alchemical philosophy" Chemists see no manifestations of a tendency of being able to convert lead into silver, or silver into gold These metals are not chemically *conformable,* one cannot take place of another by substitution, they do not form an isomeric group

The preceding illustrations are drawn from inorganic bodies Chemists have long believed that certain *organic* compounds display, in their properties, a close resemblance to metals Of this kind are the three organic radicals—

$$\begin{array}{lll} C & H_3 & O \\ C_4 & H_5 & O \\ C_6 & H_7 & O \end{array}$$

which may be regarded as the three several oxides of an isomeric triad, bearing analogy to those already adverted to in the inorganic world With reference to these radicals (omitting the oxygen) it is found, as in the case of the inorganic triads, that the sum of the atomic weights of the extreme bodies divided by two is equal to that of the intermediate body

---

* See Chemical Record July 12 1851 and Lectures on the Non Metallic Elements, p 160, by Faraday

The discovery of allotropism has bereft these speculations of impossible extravagance The allotropic modifications of which sulphur, phosphorus, oxygen, and carbon, are susceptible, suggest the thought that the countless array of organic compounds which now bewilder the chemist, may prove only modified forms of one unchanging radical Like those of isomerism, the phenomena of allotropism inspire the zoochemist with new hopes

Solidified albumen and fibrine are allotropic conditions of liquid albumen and fibrine Physically the former differ remarkably from the latter, chemically they are identical "There was a time when the doctrine which supposed the convertibility of metals was opposed to known analogies, it is now no longer opposed to them, but only some stages beyond their present development" (Faraday) These discoveries have been accomplished, not by the magic touch of the philosopher's stone, but by the touch of genius

Coincidently with these recondite speculations as to the transmutability of *matter*, have been projected novel thoughts as to the "mutual correlation and convertibility of '*forces*'" (Grove) Heat, light, electricity, magnetism, motion, &c, are severally interproducible "forces" (Faraday) "Vital action," cell growth, nerve-force, muscular action, *are the* physical imponderable forces modified in manifestation by passage through an organic material substratum (Carpenter) Heat becomes vital force by passing *from* without *into* the egg Such expressions imply the locomotion of an entity Against the vague use of this hypothesis involving-language, the student of organic chemistry should jealously guard He must be warned once more, lest he confound the *ego* with the *non ego*—the conditional with the absolute and unconditioned All general ideas are born of abstraction They should be permitted in science only as intellectual guides The "forces" of matter are the "properties" of matter The quality cannot be separated from the body The word "property" in chemistry should be accepted in a phenomenal, not entological sense The dynamics of matter cannot be studied apart from the statics Though, however, the student be warned against the idolatrous worship of all bodiless essences, though it be affirmed that the physical philosopher is in pursuit, not of fictions, but facts, not of subjective creations, but objective realities, it must be understood that the intellectual "aid to pursuit" to be drawn from an intelligently conceived theory is not to be contemned A "theory" is not an illusive palace raised by fancy, it assists in two ways, it guides the mind in the marshalling of novel phenomena under known analogies—and it dictates method, suggests conditions, devises experiments It ought not to trammel the freedom of research, nor circumscribe the range of just speculation It is a mental light, an intellectual instrument of inquiry It is the bright path projected by forethought into tracts of research yet unsurveyed by the eye of science, and uncatechized by experiment

"Chance" may have imparted motion to the classic chandelier in the Cathedral of Pisa, but if chance also suggested to Galileo the laws of the pendulum, it must have belonged to that multitudinous order of casualties by which ideas are ordinarily propagated in fit and fertile minds Theoretic previsions had qualified the mind of the philosopher to evolve an immortal principle out of an accidental event, to generalize a "law" on the basis of an accident. "Facts," uninterpreted by "theory," are inert, incoherent verities.

A *vital fluid* is different in no single particular from any cosmical compound fluid. The constituent elements of an organic fluid completely inter-blended in the living organism, manifest the same laws and the same properties with those distinctive of them while yet within the region of inorganic matter. In the body they acquire no *new* properties. The *old* are only modified, intensified, neutralized, co-ordinated with, or subordinated to, "conditions" of action known nowhere but in the living body. An organic substance may be composed of five different elements. Each element may be present in a great number of equivalents. The number of the elements added to the number of equivalents explains the complexity of organic compounds:—thus,

	Carbon Atoms	Hydrogen Atoms	Nitrogen Atoms	Sulphur Atoms	Oxygen Atoms	Total Atoms
Albumen	216	169	27	2	68	482
Caseine	288	288	36	2	90	544
Fibrine	216	169	27	2	68	482

A "force" exerted in *one* direction—i. e., between *two* elements—unites them in the closest and strongest manner. The result is a compound which is gifted in the highest degree with the power to resist the causes of perturbation. To this compound if one more element be added, the uniting force is reduced to one third—it is now exerted in three different directions; if a fourth constituent be added, each line of force will be four times less than the sum of the power by which the first two elements were joined. If this reasoning be prosecuted to the limit of 500 atoms, it admits of no doubt that the power will remain the same in *nature*, though multiplied in *directions* of action. The centre of force remains unchanged, though the radii therefrom may diverge to numerous points of an embracing circumference. Where, then, is the limit to be inscribed between vital and cosmic chemistry, between organic and non-organic affinity? Is it at 8, or 10, or 20, or 500 equivalents? Is it at the fifth element, or the fiftieth? Is it at carbonic oxide ($C_2 O_2 = 28$) and cyanogen ($C_2 N = 26$), or at starch ($C_{12} H_{10} O_{10}$) and sugar ($C_{12} H_{11} O_{11}$)? Where between oxalic acid and those composite products containing sulphur and phosphorus, can the chemist mark the bound beyond which the physical force does not ascend? In the formation of chemical compounds, "organic principles," it is quite certain that the supposed line cannot be drawn which is said to divide physical from vital affinities. Thus, then, the instability, the ready decomposableness, of organic compounds, is a property which flows from the multiplied directions in which the uniting power operates upon the constituent atoms. In an atom of sugar the attraction radiates in 36 different lines—in one of olive oil, in many hundreds. But this denotes merely a difference of *number* in the combining elements. Is there no difference of *kind*, of arrangement? Without adding or withdrawing any element, we may conceive the 36 simple atoms of which the atom of sugar consists to be arranged in a thousand different ways, with every alteration in the position of any single atom of the 36, the compound atom ceases to be an atom of sugar, since the properties belonging to it change with every alteration in the manner of the arrangement of its constituent atoms It will be subsequently proved that whatever may be the groupings of elementary atoms, no *new* force will be required to explain their altered disposition There is, however, in the study of organic structures, a point at which the evidences of a *new* power are encountered It is in accordance with analogy to suppose that albumen, fibrine, gelatine, &c, owe their formation to the play of purely chemical affinities The power which moulds a cell, configurates a fibre, shapes the tubularity of a vessel, transcends the conceptions of the pure chemist It cannot be experimentally catechized, it cannot be imitated by any artificial arrangement It is a force *sui generis* The corpuscles of the blood assume the discal figure, not mechanically, in virtue of a rotatory or elliptic motion like the planets in the orrery In the latter case the *figure* may be predicated from a calculation of the attractive and centrifugal forces The oblate spheroid is a *necessary* figure In the attempt to explain the *form* of organized solids, all mathematical and mechanical conceptions utterly fail Wherefore the discoidal shape of the blood-cell? why the elliptical in the fish? *This is the vital force,* chemistry compounds the materials, vitality chisels them into form Chemistry unites the elementary atoms into composite groups, vitality utilizes these compound groups, and no other, in the fabrication of the solids The floating cells are the only parts of the fluids which, in this sense, exhibit the evidences of a vital force The hæmatosine and globulin and cell walls are chemical products "Vitality' gives to these products a special arrangement but no other 'principles" could be shaped, even by the vital force, into corpuscles It requires conditions of action as well as every other force There is, therefore, some mysterious relation between the *shape* and the *substance*, between the *material* and the *form* The cells of the blood, though the constructive principles were provided, can be formed nowhere but in the blood—no other locality offers the required conditions Muscular fibre can only be formed out of previously prepared compounds, and *then* only in a particular place Thus obviously governed by "conditions," the *vital* force, as much as any other force, becomes a legitimate object of investigation Allied intimately to the chemical force, it discovers a mechanical and mathematical mode of operation In this inquiry the difficulty has hitherto proved insurmountable, of ascribing effects to their true causes—of explaining the share which belongs to each "force" in the arrangements which constitute life

The organic *fluids* fall exclusively under the dominion of chemical laws Under chemical circumstances, the atoms of bodies are more inclined to unite in simple than complex numbers These combinations are always regulated by definite proportionality Organic compounds do not always obey this law The union of mineral bodies with organic principles is often governed by no proportionality Phosphate of lime in union with osteine is regulated by no limits It is possible to separate a large part of the non-metallic element contained in the protein compound without destroying the distinctive characters of the principles It is not yet certain whether these elements in albumen and fibrine are chemical or mechanically mixed. The protein compounds have not yet been separated and demonstrated in a chemically pure state Albumen is intimately blended with the salts of soda That they are chemical compounds *at all*, cannot, in the present state of organic analysis, be affirmed with confidence The elements may be held in union by a homogeneous, not heterogeneous force They may yet prove to consist of peculiar mixtures, rather than chemical compounds

In the arena of organic chemistry the *elementary bodies*, as such, enact only a subordinate function Oxygen, in its unmixed form, cannot be "organoleptically," histogenetically, employed under any circumstances Nature fabricates with compound atoms, not with simple The simples first unite to build compounds—*then* they are qualified The composite atoms react upon composite atoms, and these upon others With each complication the chemical force is replaced more and more by another, which undoubtedly *grows out of*, *is directly developed from*, the former *Chemical* force is inversely as the atomic weight, *vital*, directly This is a beautiful generalization The chemistry of organized beings is the chemistry of compound bodies, not of simple The solids can only be constructed by means of compounds In the fluids the *elements* are arranged into compounds The chemistry of the fluids concerns itself chiefly with mineral bodies and their unmodified properties, that of the solids exclusively with peculiar compounds The individual atoms of bodies are *in motion* in the organism from the instant of entrance to that of exit The momentum acquired by such a motion impels them to that *mode* of union which is distinctive of organic substances Vital force appears thus as the higher evolution of the chemical Whether, with Newton, Guyton, Morveau, and Buffon, chemical affinity be regarded as due to "universal attraction," or with Berthollet, to an attraction of "a peculiar nature," or with Berzelius and Sir H Davy, to the electrical properties of bodies,—it is certain that chemical substances unite and disunite, move and fix themselves, *in* the organism and *out*, in obedience really to the same laws

The fluids of the higher animals are more complex than those of the lower, the solids are so also The non-proportional constitution of albumen, as will afterwards appear, favours the ready renewal of the fluids in the case of the lower invertebrata In the blood of the higher animals, albumen, fibrine, and the oils, by endowing this fluid with a thickness and viscidity, diminish the rapidity of molecular movements, and the freedom with which, if swayed only by their chemical affinities, the mineral constituents would interchange places *Viscidity* becomes thus a mechanical condition capable of imparting a certain measure of permanency and stability to the composition of the blood This quality is due much more to the fibrine than to the albumen The fluids of the lower invertebrata are almost entirely devoid of fibrine, they are much thinner, less viscid, and of far lower specific gravity The lower animals *live faster* than the higher, the operations of life are more rapid, and briefer in duration Complex compounds demand for their production more time than the simpler Hence the harmony between the physical characters of the organic fluids respectively in the lower and higher animals, and the *rate* of the nutritive operations, the durability of the resulting products, and the ' term of life "

Organic fluids differ from inorganic in a marked manner in the greater frequency of the phenomena of contact or catalysis A feeble acid will change starch into glucose, it excites without participating in this mutation Hydrogen and oxygen left to themselves, even in presence of strong sulphuric acid, will not combine—spongy or leaf platinum will instantly initiate that combination, this is a phenomenon of simple contact, catalysis Pure sugar in solution will remain unchanged—added, dilute sulphuric acid, it appropriates water in the proportion required to form glucose, the acid remains unaltered Caseine or gluten placed in a solution of glucose, the latter will transform itself into lactic acid, without taking anything from the former Sulphuric acid mixed with alcohol produces sulpho-vinic acid—*this* distilled, gives ether and sulphuric acid—the acid is unchanged, this is *not* catalysis (Liebig), the acid takes a chemical part in the process

All azotized substances exposed to the air will absorb oxygen, and reject carbonic acid, if mixed with sugar in solution, the latter will be transformed into alcohol and carbonic acid, the azotised matter in a state of change cedes nothing *material* to the sugar—but to its atoms it gives *motion* Berzelius contends that the passive contact of the ferment initiates the fermentation, Liebig holds that the motion of the atoms of the ferment propagates itself to those of the fermentescible body Catalytic excitants may cause the union as well as the *dis*union of elements Oxide of silver *de*composes per-oxide of hydrogen, platinum determines the *com*bination of oxygen and hydrogen, the word catalysis is, therefore, not strictly applicable Faraday has recently projected the splendidly simple thought, that the part of the catalytic agent is really *electrical*, it acts as a conductor of electricity between the two uniting bodies, thus in science has genius dissipated another arcanum The theory of catalysis finds frequent illustration in the metamorphoses of organic compounds, its application will be hereafter shown

The very essence of zoochemistry may be thus stated *in summa, like produces like*—organic principles produce organic principles This may be called homologous catalysis, organized beings do not, cannot create, they cannot transmute elements they can only undo, make anew, construct compounds out of pre existing simples The vital fluids, unlike inorganic solutions, never subside into the quiescence of chemical and molecular equilibrium, the union and disunion of the elements are ceaseless, the play of affinities is complexly various, the blood is, nevertheless, an organic unity No single element can be withdrawn without destroying that unity Then how is this wonderful fluid to be studied? If dried and burnt in total substance, and the *ultimate* products collected and weighed, the whole might readily be tortured into the fantastic shape of a chemical equation, or by an analysis less extreme, all the bases, and acids, and organic compounds, might first be determined, and then an empirical formula might be constructed, expressive of their relative positions little advantage would result from either of such methods An exclusively chemical or analytic investigation will prove alike unproductive, the purely physiological is no less inadequate, assistance must be drawn from both departments of study The inquiry, then, must embrace the following questions —1 What are the *elements?* 2 What are the *groups* which are formed by the union of these elements? 3 How are these groups (compounds) related among themselves? 4 *How* and where do they become parts of the solids? 5 In what manner do they return into the current of the fluids, and finally escape as *excreta?*

It is indispensable to the intelligent pursuit of this inquiry, in this place to demand by what *names* are the constituents of the blood to be distinguished, their *chemical* designations will of course remain, but is albumen to be called an "element" or a "principle"? An element is an undecomposable, indivisible substance. Albumen, though a chemical compound, is an organic atom, take *any* part away, and it ceases to be albumen. Is oxygen a "principle," or an "element"? It is no real part of the blood until it enters into chemical union with some one of its constituents, it must surrender the gaseous and assume the fluid form. In the state of gas, it is literally an extraneous body, it is only in readiness *to be* used, it nowhere constitutes an anatomical part of an organized substance, it is in itself nothing, it must first merge its individual properties in those of the compound of which it is to form an element, it enters into chemical union with almost every constituent of organized beings. But is it, because it can be detached again by violent means, extracted with all its original properties, to be called a "principle"? If it be denominated a "principle," it will lead to endless confusion to call also a "principle" the compound, of which it is but an element. What is sulphur out of its place in the protein compounds, whether there oxidized or not? How different its organic value in the sulphates! What is nitrogen out of its position in the azotized groups? In the language of Chevreul* it would be called a "*mediate* principle," and the compound into which it enters, an *immediate* principle. In the recent work of Robin and Verdeil,† these acute authors distinguish both the element and the compound as "*immediate principles*." On what ground? On that, that *all* substances which can be extracted from the animal body "without decomposition," are entitled to be classed as immediate principles! Thus the chemically undecomposable, and the organically undecomposable, are alike to be called "immediate principles!"

Oxygen is not a histogenetic substance, it expends its force upon the fluids. Here the organic compounds are formed, from which the solids are afterwards constructed. These observations apply with undiminished force to the *salts* concerned in the production of the organic compounds. Let it be supposed that oxygen, hydrogen, nitrogen, and carbon, were so grouped as to form an "organic radical," it could not exist as such in its pure and unmixed state, it must be fixed either by chemical union or mechanical alliance with an inorganic body. Lime is united to osteine in the bones, potash to musculine in the muscles, soda to albumen in the blood, phosphorus to neurine in the brain, &c. The living body is thus not only "a superstructure of physics on chemistry" (Arnott), it is a fabric raised of organic compounds on the basis of mineral bodies. Then, are the elements, oxygen, hydrogen, carbon, and nitrogen, which constitute the very substance of a protein compound, the compound itself, as well as the mechanically allied mineral body, indiscriminately to be ranked as "immediate principles"? Are creatine and urea, which are *out*going, and albumen and fibrine, which are *in*going substances, alike to be termed "immediate principles"? Is the answer, that they acknowledge a diversity of source, that they discover differences of properties, and tend towards unlike destinations, of valid avail? What in the living organism is *not* of mineral origin? Is the oxygen inspired, in breathing, directly from an inorganic source, forbidden to form part of the protein compounds in the blood? If not, why should not the albumen of the serum be indicated as a "principle of mineral origin"? It is augmented by an element immediately drawn from without. Oxygen, hydrogen, nitrogen and carbon, are really the only elements essential to the production of 'organic principles." The acids, the earthy and alkaline bases, and the non-metallic bodies, are accessory, though important constituents.

---

* Récherches sur les Corps Gras d'origine Animale. Paris, 1803, and Considerations sur l'Anat. Organique. Paris, 1824.

† Traité de Chimie Anatomique et Physiologique Normale et Pathologicale, &c. Paris, 1853. Though I may lament the diffuse verbosity of this work, I would yet signalize it as one which I have studied with the greatest advantage. Less cautious than Lehmann, in their very original work Robin and Verdeil discover more genius

Let, then, the word "element" retain its legitimate chemical signification; let the oxides, acids, and salts be denominated "mineral compounds," and let all adipose and nitrogen products—whether progressive or regressive, whether crystallizable or not—be ranked in the chapter of 'organic *principles*": what, then, does the blood contain?

1000 *parts of blood corpuscles contain*

Water	688.00
Solid constituents	312.00
Specific gravity 1.0885	
Hæmatin	16.75
Globulin & cell membrane	282.22
Fat	2.31
Extractive matter	2.60
Mineral substances (without iron)	8.12
Chlorine	1.686
Sulphuric acid	0.066
Phosphoric acid	1.134
Potassium	3.328
Sodium	1.052
Oxygen	0.667
Phosphate of lime	0.114
Phosphate of magnesia	0.073

1000 *parts of liquor sanguinis contain*

Water	902.90
Solid constituents	97.10
Specific gravity 1.028	
Fibrin	4.05
Albumen	78.84
Fat	1.72
Extractive matters	3.94
Mineral substances	8.55
Chlorine	3.644
Sulphuric acid	0.115
Phosphoric acid	0.191
Potassium	0.323
Sodium	3.341
Oxygen	0.403
Phosphate of lime	0.311
Phosphate of magnesia	0.222

This tabulated view of the quantitative relations of the principal constituents of the blood is founded upon the analyses of Lehmann* and Schmidt.†

All the constituents of the solids must once have existed in the fluids; the analysis of the former would not, however, express the composition of the latter. In the solids, the organic and inorganic compounds enter into different and new combinations. Though in ultimate analyses these two parts of the animal body present a striking resemblance, in proximate they widely contrast. Many of the saline compounds discoverable in the living fluids exhibit the same properties here as in lifeless solutions; they obey the same chemical affinities. The chlorides of sodium and potassium, the carbonates and bicarbonates, sulphates and phosphates of lime, soda, potash, magnesia, and ammonia, though always in solution while in the organism, may yet be separated from the fluids by crystallization and volatilization. That chemical or organic compound which admits of extraction by either of these proceedings *without* decomposition is entitled to rank as a component "principle" of the fluid or solid from which it was derived. With Robin and Verdeil it is important to distinguish between those bodies which are drawn ready-formed from external sources, from those which are products of the chemistry of the body: to the latter class belong the urates, hippurates, lactates, oxalates, isonates, pneumates, creatine, urea, sugar, &c. They exist only in organized bodies, to the agency of which they owe their formation, and from which many of them are rejected externally, while others are resolved into their elements while yet in the organism; they sink by decomposition into simpler and simpler bodies; they are on their passage out of the organism.

---

* Physiological Chemistry, translated by Day, vol. ii, p. 160.

† Characteristik der Cholera, quoted by Lehmann.

The living fluids comprehend another class of constituents, to the presence of which the former owe their distinctive qualities—viz., albumen, fibrine, and the fatty principles. It is from these that the organic bases of all the solids are derived; osteine, musculine, globuline, cartilagine, &c., are really the former, modified by altered chemical conditions. In the organism they exist in the fluid or semi-solid form; they are neither crystallizable nor volatile without decomposition; in chemical composition they are non-proportional; they constitute the essential substance, or material substratum, of all organized bodies; organized beings alone offer the conditions essential to their formation. The act of solid nutrition may be almost defined as that of the transition of these principles from the fluid into the solid state.

Under the head of the fatty principles of the fluids will come hereafter to be examined the saponifiable fats (margarin, olein), the phosphorized or non-saponifiable fats, and cholesterine and serolin. The analyses of Becquerel and Rodier have proved that these exist in the blood. Berzelius has shown, what Lehmann has confirmed, that the phosphorized fats (analogous to the glycero-phosphoric acid discovered by Gobley in the yolk of egg) accumulate principally in the blood cells. The cells of arterial blood are poorer in fat than those of venous. Fat also exists in the liquor sanguinis, united intimately but mechanically to albumen and fibrine. In the fluids of the invertebrata the adipose elements are present in far greater relative amount than in those of vertebrated animals. Professor Quekett has affirmed the general principle, that the 'adipose cell' does not exist in invertebrate animals. Neither of the solids nor of the fluids is this quite true. The floating cells abounding in the chylaqueous fluid of the acalephæ, echinoderms, and especially of the annelids, are *chiefly* laden with fat; they *secrete fat* as literally and obviously as the liver-cells of the cod. "Fat certainly does exist in insects, crustacea, and mollusca, but no true adipose cell is ever present; it could not be nourished without its accompanying bloodvessels, and these are not found in invertebrata. The tissue resembling adipose tissue usually belongs to the liver or other glandular organ, and the fat exists in its cells in the form of oil."* From this passage it is evident that Professor Quekett has incorrectly observed and erroneously interpreted the mechanism of nutrition in the invertebrated animals, in them the solids are penetrated and pervaded as intimately by the fluids as in the example of the vertebrated animals. The adipose tissue in insects is bathed and traversed by fluid, the walls of these areolæ *secrete* the fat from the circumambient fluid, though they do not constitute "*closed* cells," they notwithstanding perform the functions of adipose cells, but it is quite extraordinary in what abundance the adipose principles accumulate in the *hepatic* cells of the invertebrata! If these cells were found floating in the fluids they would be undistinguishable from those morphic elements which form the normal constituents of the latter. Fat, then, enacts a constant and prominent office in the secernent agency of cells, alike fixed and free, in the invertebrata. Upon the basis of these facts, developed under the guidance of logical analogy, the argument may be reposed which contends that the cells of the blood, though not sedentary, should physiologically rank as true glandular agents. In the invertebrata, fat is transferred in great part from the solids to the fluids—*fibrine* and its modifications from the fluids to the solids. Schultz and Schmidt have recently supposed that fibrine occurs as a *separate* principle in the following manner: "As the blood escapes out of the body, an acid albuminate of soda which is dissolved in it becomes disintegrated into its component parts in such a manner that a less acid, neutral or basic albuminate of soda remains dissolved, while the other atom of albumen separates under the form which we name fibrine and fibrine subsequently contracts to the smallest possible volume—just as freshly-precipitated silica, alumina, and phosphate of lime, gradually contract."* The researches to be subsequently noticed in these papers will constrain the physiologist to adopt one of two conclusions: either that in the fluids of the *lowest* invertebrata fibrine has no existence as a separate principle, or that, on the hypothesis of Schmidt, those chemical conditions do not occur on which the separation of fibrine as such from the fluids depends—a conclusion which implies that fibrine and albumen are essentially identical bodies.

---

* Lectures on Histology, by Professor Quekett, p. 182.

Insurmountable difficulties will oppose the study of albumen as a "principle" isolated from the saline ingredients of the serum. They are inseparably united, they accompany one another in their respective physiological acts. Scherer has shown that the apparent properties of albumen vary according to the different proportions of alkali or salts with which it may be combined, the organic group of atoms in the albumen remaining unchanged. The neutral, basic, and acid albuminates of soda are marked by differential chemical properties. The several forms in which albumen coagulates will be found to be due to the varying amount of alkali with which it may be combined. Albumen stands in a far more intimate relation to the floating solids of the fluids than any other principle. It is *never* absent, it plays in the chemistry of organized being a primary and fundamental part. Albumen is remarkable in this respect,—that it is capable of entering the blood of even the higher animals unchanged by the agency of the gastric chemistry. Albumen *peptone* is more like albumen than fibrine-peptone is like fibrine. To this fact extreme interest will attach in the study of the origin and composition of the nutrient fluids of the inferior invertebrata.

The *mineral* constituents of the *serum* of the nutritive fluids occur in the animal kingdom under remarkable diversities. In those inferior animals in which the circumfluent element is admitted *directly* into admixture with the nutrient fluid, the saline ingredients of the serum are identical with those of the surrounding medium. In the blood of the vertebrata the phosphates and potash salts predominate in the corpuscles—the soda salts, and especially the chloride of sodium, in the serum. The alkaline sulphates and carbonates exist in simple solution in the water of the serum. In the blood a considerable amount of carbonic acid exists in combination with alkaline bases. The methods of analysis used by Professor Rose have ed him to the conclusion that the various compounds of sodium, potassium, calcium, iron, phosphorus, and sulphur, exist pre-formed in the organic substances in an *unoxidized* state, and take an important part in their metamorphoses. The carbonate and phosphate of lime are important ingredients in the fluids of the invertebrata. The polypidoms of the Polypifera, and the shells of the Mollusca, are composed almost exclusively of carbonate of lime, the calcareous casing of the crustacea, on the contrary, are formed as exclusively of the phosphate. It will be interesting, subsequently, to ascertain whether the fluids respectively in these classes present corresponding diversities.

---

* Lehmann's Physiological Chemistry, vol. ii. p. 196.

The ash of all animal fluids abounds in the *phosphate of magnesia*, it is present in large quantities in the cerealia, hence its predominance in the blood of the herbivora. The intestinal concretions of herbivorous animals are composed of this salt. In the horse they consist of the triple phosphate. Like carbonate of lime, the functions of phosphate of magnesia in the organism are probably mechanical.

Chloride of sodium is an important constituent of the blood. To this salt albumen owes much of its solubility. It is capable of impeding the coagulation of fibrine, and of acting as a solvent for caseine. Distinguished from other inorganic bodies, its combinations are regulated in the organism by definite proportionality. Its proportion in the blood, under normal circumstances, is *constant*, in disease it varies. It is the source of the hydrochloric acid of the gastric juice. Boussingault has determined that the presence of this salt in the blood facilitates the deportation of excrementitious substances from the body.

The *alkaline carbonates* abound in the blood. In that of the higher animals those of soda preponderate, in the lower, those of lime. The bicarbonate of soda is capable of exerting a special power over albumen, augmenting strikingly its solubility—of impeding the coagulation of and chemically dissolving fibrine. Soda supplies a base for the acids formed in the fluids. The carbonates do not reach the blood as such, they are formed from the lactates. Upon their presence chiefly depends the alkalinity of the blood. This property of the nutritive fluids knows no single exception in the animal kingdom. The fluidity of the blood flows from its alkalinity; the walls of the bloodvessels are impermeable to an albumino-alkaline fluid. The free alkali acts as a resistance to many causes which, in the absence of the alkali, would coagulate albumen. Liebig has proved that the more alkali the blood contains, the higher the temperature at which albumen coagulates. The oxides of iron and other metallic oxides, are rendered readily soluble in the blood by the action of alkali. The latter also exert upon the former the extraordinary influence of depriving them of their characteristic colours, such that a perfectly transparent solution results This interesting fact explains the *colourlessness* of the fluids in the invertebrata To this rule the blood proper of the annelida *only* is exceptional The chylaqueous fluid of zoophytes, acalephæ, echinoderms, and annelida, contain iron, they are yet devoid of every trace of colour The blood of molluscs and crustaceans is perfectly transparent, it yet affords ready indications of the presence of iron In the *fluids* of the invertebrata, it may be stated as a rule, that the metallic oxides and their compounds exist under a colourless form; the cells of the *solids* exclusively are endowed with the pigment-forming power

The blood of the vertebrate animal contrasts with that of the invertebrate in the abundance of its hæmatosine This is the most palpable difference, other less striking, but not the less essential distinctions, divide the fluids of these two great kingdoms One may be here stated by anticipation in the fluids of the invertebrata, *one* order of floating corpuscles only exist, in the blood of the vertebrata, *two* are readily distinguishable

The free alkali of the fluids serves also to promote the combustion of organic compounds, which in its presence acquire a power of combining with oxygen, a property which they do not possess at ordinary temperatures, thus, milk sugar and grape-sugar, in presence of a free alkali, and with the aid of a gentle heat, deprive even metallic oxides of their oxygen It is by the alkalinity of the blood that the metamorphosis of the malic, citric, tartaric, and other organic acids, used as food, is promoted, and the same influence is exerted even over uric acid, which, when introduced into the system from without, is speedily resolved into urea and oxalic If, on the other hand, there be not an adequate supply of alkali in the blood, some of the vegetable acids (such as the gallic and tartaric) pass through it unchanged, and reappear in the urine this is especially the case in carnivorous animals, whose food abounds more in the alkaline phosphates than that of herbivorous or omnivorous animals *

The *alkaline phosphates*, especially that of soda, exist in considerable relative quantities in the blood The analyses of Rose render it doubtful whether the phosphates are present really as such in the blood, they are probably products of incineration Liebig maintains that the alkalinity of the blood depends more upon the existence of the phosphate than the carbonate of soda The solubility of carbonic acid in the blood is remarkably increased by the presence of the phosphate of soda Phosphate of potash does not possess this singular property, when phosphate of potash is brought into contact with the chloride of sodium, phosphate of soda is formed, this remains in the blood while the potash salt is appropriated by the muscles Thus Liebig explains the constancy with which the amount of phosphate of soda in the serum of the blood is maintained under all variations of food Potash also exists in the fluids in small proportion In the blood of the fowl it was discoverable by Liebig in large quantities, in that of mammals it is inappreciably minute Ammonia does not exist in the fluids, it is the characteristic of the excreted matters The *alkaline sulphates* play an insignificant part in the composition of the fluids The iron of the blood will be afterwards studied in connexion with the corpuscles

---

* Researches on the Chemistry of Food, by Baron Liebig pp 115 116

Thus a brief sketch has been presented of the history of the protein-compounds, oleaginous principles, and inorganic bodies, which conspire to form the living fluids At a subsequent stage of this inquiry, each of these divisions will be studied in detail, at this, general statements only are required, they will serve to bound the field of research, and to indicate the path of investigation

The *floating cells or corpuscles** in chemical and proximate composition, differ remarkably from the fluid by which they are borne, this fluid may be indifferently distinguished as "the *fluid*," the *liquor sanguinis*, the *intercellular fluid* (Lehmann) The cells are the floating solids of the fluids, they are determinately organized structures, as truly so as any other living solid Deriving from the liquor sanguinis their elementary parts, with it they yet contain scarcely a single principle in common Though thus contrasted in proximate composition with the sustaining fluid, to the normal existence of the latter they are yet essentially necessary Between these two elements of the blood there obtains an intimate mutuality of action and reaction No single real example occurs in the whole animal kingdom of a nutrient fluid totally destitute of morphological elements This rule is ultimate and absolute.* The origin, and office, and destiny, of these elements are time-honoured enigmas in science, which physiology has attempted in vain to unriddle A new method of investigation will consist in modestly interpreting the experiments practised by Nature's own hand Let the mind contemplate first the incomplex fluids of the humblest organisms, it discerns living fluids of the simplest chemical composition, and of less entangled relations to the organized fixed solids These are circumstances which favour the solution of the refractory problem which relates to the mode in which the morphic elements of the fluids arise, grow, and disappear These are questions which science has never answered Theories are confusingly numerous, demonstration is utterly wanting A correct apprehension of the conditions which determine the origin and propagation of the moveable organized solids of the blood, will lie at the root of all future discoveries in histogenesis The causes inciting to *their* formation must prevail at the birth of every other organized solid, sedentary or free The act of organization implies the transition of a soluble substance into an insoluble, of a liquid into a solid, the material is first precipitated, *then* shaped into form, chemistry and physics preside over the scene The constituent parts of the nutritive fluids, when traced from the base to the summit of the animal series, occur in the following order of frequency —(*a*) water, and the saline substances dissolved, (*b*) albumen, (*c*) corpuscles, (*d*) oleus principles, (*e*) fibrine Albumen is the first *organic* principle which is superadded to the inorganic saline solution, constituting the basis of *all* vital fluids Corpuscles appear next, then the fatty principles, and lastly the fibrinous If the corpuscles were literally an evolution of the fibrine in the line of a supposed progressive metamorphosis, *their* presence would imply necessarily that of the fibrine, the converse will prove to be true. In a limited number of instances, albumen occurs *without* corpuscles, these latter *never* without albumen The history of the fluids in the animal kingdom will be prosecuted with more exactness and success, if first that of the floating cells be made the subject of separate and special investigation

---

* It recently occurred to me while prosecuting certain inquiries into the mode in which the corpusculation of the animal fluids takes place to imagine that *vegetable* fluids would exhibit this process under terms of greater simplicity Reducing hypothesis to demonstration my first observations impressed me with unbounded surprise I found at once *that the milk sap of different plants was charged with floating globules of different size and conformation that those of different plant fluids are variously affected by the same reagents* I claim no novelty for these observations Schultz (Die Natur der lebenden Pflanze 1823—28 and Sur la circulation et sur les vaisseaux laticifères dans les Plantes 1839; and also Die Cyclose des Lebensaftes 1841) has diligently laboured in this field I wish only to raise the subject to the eminence of a new point of view Schultz saw in the globules bearing milk sap of vegetables the counterpart of the blood of animals Hugo von Mohl ridicules this reputed analogy. The ridicule may yet redound upon the scoffer The recent researches of MM Melsens and Panum on the corpusculation of artificially prepared fluids render it almost certain that the plant fluid *globulates* under the government of the mechanical conditions of *pressure* and *motion* (For an account of these researches see the admirable report by Dr Lyons which appeared in the April number of this Review) Since the globules vary in the fluids of different species of plants, a diversity in the chemical composition of these fluids is implied In a subsequent number of this Review I will state at length the results of my inquiries into the history of the *corpuscles of the vegetable fluids*

HISTORY OF THE MORPHOTIC ELEMENTS OF THE NUTRIENT FLUIDS IN THE ANIMAL KINGDOM

It were here superfluous to enunciate the physiological canon, that in the lower organisms the processes of fluid and solid organization are less complex than in the higher The elements concerned are fewer in number—of the fluids this remark is especially true The more simple the fluid, the more comprehensible the reactions which occur between its constituents Problems too involved to admit of solution through the study of the blood of the highest animals, assume an intelligible, because less complex, expression in that of the lowest This is the ground on which it is preferred to commence the present inquiry at the lowest link in the chain of animal life. Among the invertebrated animals, *two orders* of fluids occur, which are distinguished from each other in several striking respects The first is the *chylaqueous fluid*,* the second is the *blood proper* The chylaqueous fluid is always contained in the great splanchnic cavity, which is bounded on one side by the tegumentary parietes of the body, and on the other by those of the alimentary canal It moves freely in this capacious chamber, and penetrates into all the dependent cells and channels of the organism In zoophytes, actiniæ, and medusæ, this cavity openly communicates with that of the stomach, of which it forms the apparent prolongation In the echinodermata and annelida it constitutes a perfectly-closed chamber, opening directly neither externally nor into the alimentary canal A totally different rule applies to the blood proper contained in a closed system of vessels, it moves in a determinate circulatory orbit The motions of the chylaqueous fluid are those of irregular oscillation, excited in part by ciliary, but chiefly by muscular action The chylaqueous fluid presents gradations of composition, in its simplest form it is a very dilute solution of albumen in sea water The "corpuscles" have scarcely yet appeared, there prevails always a direct proportion between the amount of albumen and the number of the corpuscles It is only in the highest or most developed grades of the chylaqueous fluid that *fibrine* occurs, and *then only in very small comparative amount* True blood differs from chylaqueous fluid in this important particular—that the former never occurs in the animal series without fibrine, the latter *almost* always The blood-proper of every known animal is corpusculated but that of the annelida The blood of annelids is almost invariably coloured, pigment therefore does not necessarily depend upon the agency of corpuscles The morphotic elements of the chylaqueous fluid are, for the most part, distended with highly-refractive oil, in those of the blood-proper this principle *never* assumes a visible form Albumen, corpuscles, and fat are thus intimately associated in the simplest type, under which the nutritive fluids of the animal organism occurs These definite and incomplex facts are prophetic of discovery The chylaqueous fluid and the true blood of every animal, alike fresh-water and marine, afford an alkaline re-agency The organized solids are always acid An acid nutrient fluid is not known in the animal kingdom Facts of wide-spread application are "*laws*" Here, then, are some of them Sea-water is a rapid solvent of albumen It is a circumstance of singular interest, that the vast multitudes of the lowest forms of animals are inhabitants of the sea, salt water constitutes the bulk and basis of their nutritive fluids How wondrous the adaptation between animate and inanimate nature!

---

* I would here desire to explain that in the paper On the Chylaqueous Fluid and Blood Proper of the Invertebrated Animals' published in Part ii 1852 of the Philosophical Transactions I have given at length the history of discovery upon this subject and that although the *present papers* are founded upon *entirely new* researches they prove essentially confirmatory of the views and facts stated in the essay to which I have referred

The *fluids* have yet received no attention in those unicellular types of animal life—i e, the gregarinæ, the amœba, the proteus, and the actinophrys sol, &c —which stand below the zoophytes in the scale The semi-fluid contents of these simple cell structures must of course form at once food and blood Under this type the porifera must be ranked The animalized covering of the sponge consists of a congeries of cells, which are distended with a non-corpusculated albuminized salt water

Polypifera—Three types of structure prevail among zoophytes—the hydroidal, the asteroidal, and the helianthoidal * The first includes the true zoophytes and the hydræ, the second, the alcyons, the third, the familiar actiniæ In the zoophyte, the stomach opens directly by a sphincteric orifice into the fleshy axis of the polypidom, this is alike the case in the solitary and composite species The transparent horny walls of the polypidom are separated from the fleshy axis by a very distinct interval, which is occupied by a hyaline areolar substance The body of the polype, properly so called, consists of a stomach embraced by a tegumentary wall, these two cylinders are separated by an open space filled with a transparent fluid The axes of the tentacles are the prolongations of this space, they are penetrated by the fluid contained in the latter, this fluid bears no visible floating globules The fleshy axis of the polypidom is tubular in all hydroidal polypes, it is filled with a rapidly-moving corpusculated fluid The motion of the corpuscles is most distinctly perceptible through the walls of the polypidom and the fleshy tube itself The globules whirl in every direction—sometimes advancing, sometimes receding, now revolving in the same place, and now coursing spirally along the sides of the tube This motion was studied by Lister, its existence has been confirmed by Sharpey and Van Beneden The globules are moved by cilia this fact is now unquestionable Neither Lister, nor Sharpey, nor Van Beneden, could, however, succeed in demonstrating their presence In the stems and branches of Laomedea geniculata they are convincingly conspicuous * The floating-corpuscle so obviously present in the fluid, occupying the axes of the polypidoms everywhere, passes upwards into the space between the stomach and tegumentary parietes only when the polype fully distends itself it is seldom seen in the axeal channels of the tentacula The splanchnic cavity in some species appears to be divided from the polypidal tubes by a cribriform septum, it is then only the fluid parts which filter through, the corpuscles remain behind This foreshadows the *first* attempt of nature to form true blood out of a chymous fluid Here the corpuscles are no agents in the respiratory process, they do not penetrate the organs dedicated to this function Neither the true splanchnic chamber, the interior nor the exterior of the tentacles, are ciliated in any hydroid anthozoa The fluid in these processes differs in no sensible particular from that in the axes of the polypidoms but in the absence of corpuscles, they cannot, therefore, be distinguished as two distinct and independent fluids There is a mechanical reason in some species against the admission of the globules into the canals of the tentacles—they are too large It is probable that nature has imposed a certain limit of bodily dimensions on the cells of fluids, below which they cannot discharge the office for which they are designed The illustrations of corpuscles accompanying the present communication were drawn as faithfully as possible, as they were seen in motion in the axes of the polypidoms They constitute only varieties of one type Figs 1 to 8, Plate I, exhibit one general character, they are thick-walled vesicles, commonly spherical, sometimes compressed, bean-shaped, frequently nucleated, and now and then granular They are in some cases intermingled with highly-refractive oleous molecules These corpuscles bear the general character of inferior organization, they arise in an albuminous solution of salt water, they are invariably present, the conditions determining their production are therefore constant It will not dissipate the mystery to affirm, with Ascherson, that they arise mechanically, as though by the contact of two homogeneous fluids (oil and albumen), or with Melsens, that the presence of certain salts precipitates the albumen, or with Gluge, that pressure and agitation will account for their origin They present a definite typal character in all hydroid zoophytes There is a speciality in the process of histomorphosis to which they owe their formation Fig 1 represents these bodies as they occur in the fleshy polypidom of Clava multicornis, fig 2, Hydractina echinata, fig 3, Sertularia polyzonias, fig 4, Sertularia pinaster, fig 5, Plumularia falcata, fig 6, Plumularia catherina, fig 7, Laomedea geniculata, fig 8, Campanularia integra, all magnified about 400 diameters These corpuscles may be noted as distinctive of the zoophytic type

---

* History of British Zoophytes, by Dr Johnson 1842

In the *hydræ*, it is easy to demonstrate the existence of a fluid charged with delicate pellucid globules, in the *cells* of the space between the stomach and the tegumentary walls In the hydræ this space is not divided by parallel longitudinal septa as in the actineæ, but by intercrossing septa At the roots of the tentacles the moving globules may be readily observed, they do not appear to traverse the axes of the tentacles The perigastric space in the hydræ, as in the hydroid anthozoa, is destitute of ciliary epithelium The corpuscles partake more of the character of oil globules (figs 9 & 10) In hydra viridis, they consist of a few granular spherules (fig 9), intermixed with oily molecules Those of hydra vulgaris discover the same character, abounding more in oil In the *asteroid polypes*, the stomach of each polype is prolonged into canals in the polypidom No corpuscles can be seen moving in the tentacles when they are being protruded by the limpid fluid of the splanchnic cavity A thin section made by aid of Valentine's knife, parallel with the canals of the polypidom, and placed carefully under the microscope, will display the motion of cilia

---

* I have examined the greatest number of the species abounding along the coast between Swansea and Tenby In none have I failed to detect cilia in the fleshy axes of the polypidoms In Tubularia, Clava, Hydractina and one or two more species the walls of the polypidom are too dense and obscure to render the observation of the cilia satisfactory In others their existence cannot be disputed

The corpuscles, as seen moving under ciliary action in the channels thus observed, are represented in fig 11 They correspond with those of other zoophytes They are little organized, they consist of spherical vesicles, mingled with oil-particles Some few are nucleated, while others are charged with minute granules It is difficult to conceive the nature of the office which morphic elements so simple are capable of discharging

The tentacles of asteroidal polypes are said to be perforated at the extremities This is scarcely possible They are capable of expansion, through injection of fluid from the perigastric space into their axes

The fluids in the *Actiniæ* admit of ready demonstration Selecting the more transparent species, or young individuals, the following facts may be verified While the little object lies on its side, the fluid contained in the chambers between the stomach and integuments may be seen every now and then escaping into the stomach, through an orifice situated at the bottom of this organ The dissections of Dr Cobbold* have recently confirmed the existence of this opening The converse movement of the fluid may also be easily traced The surrounding water enters the stomach, wherein it briefly sojourns, then passes through the opening at the bottom into the great cavity of the body, in this cavity it remains for a variable period; it now injects the tentacles Corpuscles now arise in the fluid, it becomes thicker in consistence, through increase of its albumen, it is no longer pure, lifeless, salt water, it is a corpusculated, chylaqueous fluid, it is competent to subserve the ends of solid nutrition Whence do the floating cells proceed? what produces them? Certainly no solid organ, neither liver nor spleen can in this case interpose its agency Then, is it possible that there can inhere in albumen a mysterious histomorphotic power, in virtue of which it transmutes itself from the liquid into the solid condition? This were only a mode of enouncing "the theory of spontaneous generation'

Let *facts* accumulate The *true* theory of the genesis of the floating corpuscles will in time arise

In Actinia mesembryanthemum the corpuscles of the chylaqueous fluid are complex in comparison with those of zoophytes They contain secondary cells and nuclei, they are interspersed with oil globules and sperm-cells (fig 12), they are not numerous in proportion to the bulk of the fluid The cavity of the body and the interior of the tentacles, in all helianthoid zoophytes, are richly ciliated, in this particular they are distinguished from the asteroid and hydroid orders The entire bulk of the fluid in these animals, when irritated, is emitted through the mouth This faculty is peculiar to the Actiniæ In zoophytes, the contents of the polypidom are not expelled externally on the retiring of the animal into its cell The surrounding element is drawn again into the body with equal facility in the instance of the sea anemone The marvel is, that fluid thus ingested and rejected so readily should prove adequate to the purposes of solid nutrition—that free cells should develop themselves in it with so much rapidity The fluid contained in the splanchnic cavity of the Actiniæ (that is, if it have remained sufficiently long in that cavity) acquires in a very short time an impregnation of albumen, it becomes turbid on the addition of nitric acid

---

* Annals and Mag Nat Hist Feb 1853

The invertebrated animals generally are distinguished from the vertebrated in this respect,—that in the former the solids are more completely saturated with the fluids, the cells of the solids are larger, and more adapted to *retain* fluid than in the latter This is more strikingly true of those classes, the Zoophytes, Medusæ, Echinodermata, and Annelida, in which the chylaqueous fluid constitutes more or less completely the liquid medium of nutrition The inference is irresistible, that the fluids undergo a preparatory change in the interior of, or between, the cells of the solid structures, the fluids are *then* qualified for the work of histogenesis This supposition explains the simplicity, the low, almost inorganic, type of their composition, it assigns a reason for the comparative absence of corpuscles from these fluids while yet within the great cavities of the body, it accounts for the non-development of a blood-vascular system in these animals, it renders intelligible what was before inexplicable—the mechanism of nutrition in these incomplex organisms As nearly as it is possible to express in lines and points objects so delicate, fig 12 shows the corpuscles of the chylaqueous fluid of Actinia mesembryanthemum, fig 13, those of Actinia crassicornis, fig 14, of Actinia bellis, fig 15, of Lucernaria auricula all magnified about 400 diameters In morphic characters they fall within the zoophytic type

The *Acalephæ* are constituted on the pattern of zoophytes From them they differ in having no common splanchnic cavity The stomach is prolonged into canals ramifying in the solid substance of the body, the canals receive their contents *directly*, through open orifices, from the stomach Sea-water, with the organic matter, living or dead, which it may perchance hold in mixture or solution, is first drawn into the stomach, it sojourns there over the time required to subject it to the agency of the gastric secretion It is then in great part admitted into the gastric canals, here it circulates to and fro, driven by vibratile cilia Definitely organized corpuscles now evolve themselves, albumen increases in proportional amount, and the fluid becomes a nutritive compound In these canals it is submitted to atmospheric agency, from them it passes by exosmosis into the cells of the solids This fluid, in passing from the stomach into the canals, experiences the influences of no glandular organ unless those cells constituting the walls of the containing canals It signifies little whether the alimentary system be furnished with one or two openings, the character of the fluid contents remains the same In the *pulmograde* orders it may be readily collected for examination Rhizostoma Cuvieri is familiar along our coasts In this species the corpuscles of the chylaqueous fluid are large and abundant (fig 16), in their mature state they consist of compressed spheres, they bear two or three bright nuclei The whole cell is filled with minute yellowish granules, other, simple, non-nuclear vesicles are interspersed The parietes of the containing canals are internally villous, they furnish a digestive secretion, which promotes the chemical changes involved in sanguification

The corpuscles of the chylaqueous fluid of the Medusæ depart strikingly from the zoophytic type, they are much larger, they are more laden with granular and adipose contents They present a yellowish tint, indicating the dawn of a pigment-secreting property They are furnished, in the mature state, with a dense involucrum, the "molecular base" is abundant In the genera Aurelia, Pelagia, Chrysaora, Cassiopæa, and Cyanæa, the digestive canals are cæcal, as in Rhizostoma The *ciliograde* acalephs present a modified arrangement Here the digestive system is provided with an outlet as well as an inlet, the contents of the system coincide with those of the ciliograde orders The little Velella limbosa is common in the bays of our seas, it exemplifies a cirrhigrade medusan, in it the entrance to the stomach is situated at the extremity of a sort of flask-shaped proboscis, the base of which opens into a cavity of somewhat cylindrical form, which lies in the direction of the length of the body, and subdivides at each end into two prolongations, from which a system of canals appears to pass off into other parts of the body The corpuscles of the chylaqueous fluid in this beautiful medusan, like the animal, exhibit a slightly bluish tinge (fig 17) In microscopic characters they do not materially differ from those of Rhizostoma In Willsia stellata, a less common acaleph, the stomach is campanulate and opens widely by four scarcely undulated lips The surrounding ovaries impart to the colour a yellowish colour From the stomach proceed six gastro vascular canals, which subdivide as they descend to the margin of the subumbrella, and terminate in the tentacles These canals are lined with a ciliary epithelium, and filled with a fluid thickly corpusculated The floating cells are medusan in type (fig 18), they consist of cells gorged with comparatively large secondary cells, they are tinted yellowish blue Many are simple non-nuclear vesicles, the molecular base is abundant In Turris neglecta, the floating cells of the fluid contained in the gastro vascular canals are essentially similar to the former, they are, however, distinguished by the greater dimensions of the secondary cells, they dance amongst themselves, as though mutually repulsive In Geryonia appendicula, the remaining example to be here cited, the cells of the chylaqueous fluid fall under the description already given, the involucrum is dense, the secondary cellules large and oleus, the vesicles and granules numerous (fig 20)

The *acalephæ* mark the upper boundary of that division of the nutritive fluids in which the latter communicate openly and directly with the external medium The surrounding water, salt or fresh, enters immediately into the splanchnic cavity, *the* cavity always in which the chylaqueous fluid is lodged this fact constitutes a fundamental article of distinction between the fluids of the lowest animals and those of the higher In the acelephs, this splanchnic cavity assumes the shape of canals—the gastro-vascular The chambers and channels in which the higher grades of the chylaqueous fluid and the blood proper are included, are everywhere and invariably closed, neither the contents can escape, nor the circumambient element enter, in an immediate manner. From the arrangement, peculiar to, and distinctive of, the two lowest classes of invertebrata, the inference may be drawn that in *these* animals the whole *interœ* of the body may be penetrated by the external inorganic medium, no injury results from this penetration, contrarily, it forms the first condition of existence. But one striking additional fact should be stated as characteristic of this lowest division of chylaqueous fluid, *it contains no fibrine,* no coagulum forms in it, its corpuscles, which are chiefly impregnated with oleine, are the products of the histomorphosis of *the albumen,* they secrete in these degraded organisms no hæmatosine. This colouring and coloured compound is manufactured by the cells of the solids, not those of the fluids. The fact of the absence of fibrine from the fluid of the splanchnic cavity, which in these animals is the *only* nutritive fluid of the organism, there being present no trace whatever of a blood-vascular system, is corroborative of the theory, formerly stated, that the fluids acquire their final *histogenetic capacity* in the interior of the cells of the solids. It will be afterwards demonstrated that the semi fluid contents of the floating corpuscles as they exist in the true blood of the higher mollusca and crustacea is a self coagulating principle, if it be *globulin* in a colourless form, *then* globulin, like fibrine, is spontaneously coagulable. The free-cells of the fluids of zoophytes, acalephæ, are filled only with *colourless* principles, the involucra are also devoid of colour, the eye reads in their interior nothing but oleine. Ether extracts no adipose matter from the filtered intercellular fluid. The office of the floating corpuscles of the fluids in these inferior forms of life, may therefore be defined as that of converting the saccharine into the adipose principles—principles which, next to albumen, are most important in the requirements of solid organization. But let the question be now emphatically pressed—In a fluid thus constituted—of water, a few salts, albumen, corpuscles, and oleine—in what manner, and where, are the floating-cells generated? The fluid enters the stomach, sojourns briefly in contact with its *living* surface, proceeds into the interior of the peritoneal cavity, therein to oscillate, impelled by muscular and vibratile action, this is its history. In what manner, *but* through certain chemical relations into which the albumen enters, with the other principles present, where, *but* in the fluid itself, seeing that it receives the vital impress of no solid organ, can these bodies arise? Philosophic circumspection forbids at present the further flight of speculation. All that can with propriety be admitted in the glimmering light which immature science as yet affords, is, that under certain undefined conditions of development, of chemical combinations, and in presence of a living body, albumen *does* acquire the spontaneous power of passing from the fluid into the solid form. This limit bounds abruptly the light of chemistry. *Why* this "solid form," thus acquired, should, in different classes, orders, genera, and species, be differently configurated, is a question before which the genius of modern science lies powerless.

(To be continued.)

ART II

Decennium Pathologicum, or, Contributions to the History of Chronic Disease, from the St George's Hospital Records of Fatal Cases during Ten Years By THOMAS K CHAMBERS, Physician to St Mary's Hospital, London

PART IV —DISEASES OF THE HEART

THE design of the set of papers, of which this forms one, is to employ for the fixing of pathological data a consecutive series of cases examined during the ten years ending December 31, 1850, at St George's Hospital The analysis is not carried on to the present time, principally because the more recent post-mortem books are in such constant request for clinical lectures and other purposes, that to borrow them for the necessary period would occasion much inconvenience Moreover, as some day must be appointed for winding-up accounts, none seemed more appropriate than the conclusion of the decennium and the half-century, the definiteness of which date may, perhaps, induce the resumption of the task by the author or others on its future recurrence

The whole number of bodies examined was 2161 in 2539 fatal cases, of which the 378 not opened formed a perfectly promiscuous class, as is known both from the habits of the hospital, and the fact that the proportions of their ages and sexes correspond closely to those of which an autopsy was made The influences of age and sex on the whole number have been fully entered into in the 'Medical Times,' (July 24th and August 7th, 1852,) and will be repeated here only when the argument demands such quotation

Of the same 2161 cases, those will here be made the subjects of analysis and comparison which exhibited after death some lesion acute or chronic of the central organ of circulation

The condensation of materials which a course of argument by statistics allows, gives an opportunity for introducing into one paper the whole series of cardiac lesions, by which a picture of them is presented very conducive to the practical interests of truth For nothing, I conceive, leads more straightly to the gaining those broad physiological views of chronic ailments, which aid us to manage them successfully, than the union, in the mind first, and then upon the paper which is its index, of diseases which have the same real nature, while the scattering of a subject in crumbs to the winds, or the tearing it limb by limb in the volumes of a dictionary, cannot but give rise to narrow views and pedantic treatment The careful writer and the forewarned reader may and do guard themselves, but there is always this danger to be apprehended from the current literature of medicine, otherwise so beneficial to science

The chief object of the arrangement to be adopted is, convenience of perusal, but at the same time, a presumption that such is in most instances the order of the succession of morbid phenomena gives it a certain degree of natural order

A *Malformations of the Heart*

The proportionate frequency of occurrence of several of the commoner forms of *arrested development* in the heart, is probably fairly represented in the following table, but it is almost needless to say, that no implicit trust must be placed in statistics where the numbers are so small

TABLE I

Malformations of Heart	With disease also of Valves	With altered muscular walls of Heart	Heart otherwise normal	Total
Foramen ovale and ductus arteriosus open			1	1
Foramen ovale open		1	4	5
Ductus arteriosus open	1			1
Aortic valves irregular	10		1	11

Narrow as is the field of observation here presented, it is sufficient to render probable these *Deductions*

1 That patency of the foramen ovale has little, if any, influence for ill over the action of the heart

2 That irregularly formed valves are peculiarly liable to become diseased

3 That of all parts of the organ, malformation is most frequent in the aortic valves

B *Tubercular Deposit*

This was found in 2 instances in the substance of the heart, in both of which tubercle existed also in the lungs in an unsoftened condition The respective ages of the patients were 4 and 26, in the former, no change in the structure of the organ was observed, in the latter, there was a certain amount of *hypertrophy* and *dilatation* of the muscle, with *œdema* of the valves on the left side, very probably the consequences of the presence of a foreign body

The pericardium also was affected by *tuberculosis* in the first of the abovementioned cases

The pericardium alone was affected without the heart in 5 other cases In 3 of these there were *tubercles* also in the lungs, in 2 there were not In one of those, where there were tubercles in the lungs, there was *tubercular peritonitis*, and in two, *tubercular arachnitis* The respective ages of those 5 last cases were 3, 18, 20, 26, 44

Deduction—The whole number of cases of tuberculosis was 566 in the 2161 autopsies, so that the occurrence of this disease in the heart is clearly a rare occurrence, only provoked by the strongest diathesis, and at an age when general tuberculosis is most prevalent

C *Fibrous Tumour*

Fibrous Tumour is recorded as having been found attached to the pericardium in one instance

D *Malignant Disease*

In 179 cases of *malignant disease* which were examined, and at least 20 not fully examined, the heart was affected by the morbid growth in 7 instances

In 3 of these, the substance of the muscle was implicated, in 1, the mitral valve, in 3, the pericardium only

Of the first 3, in No 1, the foot had been amputated for the same disease, and the inguinal glands were affected, in No 2, the peritoneum participated in the cancerous growth, in No 3, the thyroid gland, the adjoining parts of the neck and mediastinum With the mitral valve were joined the mamma, the axilla, and the recti muscles

Of the 3 where the pericardium chiefly exhibited the malignant condition, in No 1, the pleura, omentum, ovaria, and skin were similarly diseased, in No 2, the liver, uterus, and ovaria, in No 3, the stomach, pancreas, the kidneys, and abdominal glands

In 3 of the 7 cases of malignant disease, *pericarditis* was induced by the lesion

Deductions—I As malignant disease must rapidly prove fatal whenever it attacks the heart, it may be presumed that the other organs found simultaneously diseased were its primary seat, and that its occurrence in the centre of circulation alone, is in the highest degree improbable

II It is somewhat more likely to fix in the heart than tubercle is

E *Adipose Tissue in Excess*

Hypertrophy of the adipose tissue, that is to say, an augmented amount of *normal* fat, was found,

With other chronic disease of the heart, 32 times
Without chronic disease of the heart, 17 times

In the following table are divided the *concomitant chronic diseases of the heart*

TABLE II

Chronic lesion of Heart associated with hypertrophy of the adipose tissue of the organ	Valves diseased	Valves healthy aorta diseased	Valves and aorta healthy	Total
Dilatation	5	6	7	18
Hypertrophy	4	1	1	6
Dilatation and hypertrophy	4		1	5
Soft muscle, or probable fatty degeneration		1	2	3
Total	13	8	11	32

One case where there was no chronic disease of the heart had a deposit of *recent fibrin* on the valves, the kidneys being small and granular

The chief remark to be made on the above table is, that it affords a proof that excess of adipose tissue is in itself a cause of dilatation of the heart A comparison of it with Table XX, at the conclusion of this paper, will show that in the whole number of dilated hearts, those which were so affected without valvular or aortic lesions are only half as numerous as those where the valves were diseased, whereas here, with excess of adipose tissue, they exceed them in number

We cannot be surprised at this frequency of dilatation of the heart, when we reflect on the great increase of work which is thrown on the chief organ of circulation by the increased area of capillaries through which the blood has to be propelled in fat people The addition of several stone to the weight in fat, requires certainly a very large, though not perhaps a proportionate, addition of blood and bloodvessels to nourish it, yet the same heart has still to undertake this extra labour The balance then between the systemic and pulmonary circulation must be destroyed, and the lungs be unequal to the secretion of so much more carbon than they were made for, hence the blood becomes more venous, more liable to form congestions, and to dilate the cardiac cavity by its retarded pace

The hypertrophy of the cardiac fat was sometimes associated with general corpulence of the whole system, and sometimes not The proportionate frequency of these two occurrences, together with the particular state of heart in which they were individually observed, is shown in the following table

TABLE III

State of Heart associated with excess of adipose tissue	In corpulent persons	In persons not corpulent	Total
Dilatation	13	5	18
Hypertrophy		6	6
Dilatation and hypertrophy	2	3	5
Softening or atrophy	1	2	3
Healthy state	4	13	17
Total	20	29	49

In this table may be remarked the much greater frequency with which a healthy state of heart was joined to excess of its adipose tissue when the rest of the person was normal, than when it was corpulent, an evidence that *general obesity* exercises a remarkable influence over the circulation, equally with the growth of fat on the heart itself

With respect to the 29 cases mentioned where excess of adipose tissue was found on the heart in patients of normal size, it may be observed that there is no proof that the fat there was not the remains of a general obesity, which had been reduced by the disease that proved fatal, or by other agencies For the fat is removed from the base of the heart with greater difficulty than almost any other situation As an evidence of this, it may be mentioned that one of the 29 cases was an old man of 60, emaciated by pulmonary consumption of old date, ulceration of the rectum, and abscess, yet still retaining an abnormal amount of fat in this place

The whole number of corpulent persons examined was 104 (67 of decided corpulence, 37 "stout"), of which we see 20, or 219 per cent, had excess of fat on the heart

The whole number of persons not corpulent was 2057 (i e, 2161 – 104), of which 29 at most, or 1 4 per cent, had excess of fat on the heart

Deduction —Over-development of fat on the heart is, as a general rule, part of an universal fatty hypertrophy

These last remarks are not incapable of practical application We must not forget, in treating corpulence, that fat on the heart, whether it be the last to come or not, will surely be the last to go, and we must therefore carry on the plans which we have found beneficial beyond the diminishing of the abdomen and limbs, if we would relieve the overburdened centre of circulation

F *Adipose Tissue absent*

In 340 cases of *vomicæ in the lungs*, emaciation existed in 299, but in one only of these last was the fat absorbed from the base of the heart Here it was replaced by a quantity of serous fluid effused in the interstices of the areolar tissue, so that the form of the organ and its mechanical relations to surrounding parts were in a great measure preserved This is a further exemplification of the note before taken of impediments existing to the absorption of fat from this situation

G *Acute Inflammations*

(a) *Inflammation of the Muscular Structure* —There were 3 cases of *abscess* in the muscular parietes of the heart, all arising from *purulent infection of the blood* by distant chronic diseases—viz, twice by diseased joints, and once by a bubo in the groin

(b) *Endocarditis* —Marks of recent inflammation were found in the valves in 43 instances, in 2 evidenced by *œdema* alone, in 41 by a *deposit of fibrin* *

In the 2 former there was *hypertrophy* and *dilatation* of the heart also, in one, *phthisis pulmonalis*

In the 41 where there was *recent fibrin* deposited, the immediate causes of the inflammation may be divided as follows

---

* In many cases the existence of very extensive old disease of the valve renders the description of the acute lesion doubtful, these are omitted

TABLE IV

	Cases
Uræmia from Bright's disease of kidney	12
Dropsy from diseased heart	2
Acute rheumatic fever	9
Chronic ditto	2
Chorea	1
Ditto and chronic rheumatism united, &c	(?) 1 *
Anæmia from starvation	1
Typhus	2
Pyæmia	1
Pneumonia (in one case supervening on phthisis)	3
Pleurisy	2
Malformation of valves	2
Old disease of valves	1
(?)	2

In one case the part of the endocardium just above the mitral valve was rough from *recent fibrin*, without any of the valves themselves being affected The heart was dilated, but no other obvious cause of the endocarditis is recorded

Deduction—It is clear, from the list of causes enumerated above, that endocarditis, as seen in the dead body, is either a manifestation of those diseases in which the blood is known by analysis to be the most altered in composition, or a consequence of mechanical interruption to the motion of the valve

In 30 of the above-named 44 cases the *muscular structure of the heart was chronically affected*—viz

TABLE V

Hypertrophied and dilated	15, or 31 0 per cent	
Hypertrophied	8, or 18 1	„
Dilated	7, or 15 8	„

In 2117 cases free from acute endocarditis, it was similarly diseased in 475—viz

TABLE VI

Dilated	202, or 9 5 per cent	
Hypertrophied	129, or 6 0	„
Hypertrophied and dilated	124, or 5 8	„

Deduction I—The numbers affected with disease of the heart show its probable influence in determining inflammation to the valves.

Deduction II—The comparative rank in the per-centages which hypertrophy and dilatation hold in the two last lists, show that it is the former condition particularly which has this influence

The influence of kidney-disease, or of that state of blood which is one of its conditions, in determining valvular inflammation, is sufficiently apparent from the table of cases, and has been illustrated in the paper on the subject by the present author in the April number of this journal, so no more than a reference thither is here necessary.

---

* In this instance it is doubtful how far the disease was recent or chronic

The particular valves were affected in the following proportion—viz.

TABLE VII.

	Cases
The mitral and aortic simultaneously	21
The mitral alone	14
The aortic alone	7
The tricuspid alone	1

In 3 of these cases there was chronic disease of the same valve, in 4, chronic disease of the other valves.

Deduction.—We have here evidence from acute inflammation of what will be made more certain by higher numbers from chronic states—viz., the tendency of both sets of valves to be affected simultaneously, and of the greater liability of the auriculo-ventricular valves.

This last remark would be the more strikingly exhibited, if we took only the constitutional cases into consideration: for one of the mechanical causes of the inflammation, malformed structure, has been lately shown to be almost peculiar to the aortic valves, and by adding to the number of instances of localization in that part, diminishes the prominence of the mitral.

(*c.*) *Pericarditis.*—Marks of *recent inflammation*, consisting of pus or soft fibrin, were found in the pericardium in 135 instances; in one case, the inflammation had only caused reddening and vascular injection of the membrane.

The probable direct *causes* of the inflammation were as follow:

TABLE VIII.

	Cases
Rheumatic fever	18
Ditto, with diseased heart and kidneys	1
Uræmia from diseased kidneys either alone or with diseased heart	36
Diseased heart and dropsy	18
Pyæmia	18 (or 17)
Erysipelas	1
Petechial, or typhus fever	5
Abscesses after fever	1
Pneumonia	10
Peritonitis from accidental causes	3
Pleurisy or empyema	5
Vomicæ in the lungs	8
Malignant disease in actual contact	2 (or 3)*
Ditto in the neighbourhood	1
Neighbouring abscess	2
Slough of œsophagus	1
Aneurism of aorta	2
Fracture of sternum, &c.	1
?	2

---

* In one case a general development of malignant disease coincided in time with pyæmic affection of the blood in producing inflammations after an amputation.

In this list an attempt has been made at a natural arrangement of causes, by placing as near together as possible those which appear to have the greatest likeness to one another, and by making no defined division into classes At the beginning are placed those which are most decidedly constitutional, and the most clearly local come at the end The great preponderance of the former is sufficiently evident to require no comment

As respects the ages of those who were affected with acute pericarditis, there is, of course, a considerable difference according to the cause from whence it arose

Of the 18 *rheumatic fever cases*, there were, up to the age of 21 years, no less than 1‘ 4 males and 8 females Of the remaining 6, 3 had *suppuration of the joints*—viz , 2 males, aged 25 and 45, and 1 female, aged 29, 1 male, aged 35 had also a *slough* on the sacrum, the remaining 2 were a male of 27 and a female of 30

Deduction—Rheumatic pericarditis is peculiarly a disease of youth, and when it proves fatal after 21, it is usually from some complication with other inflammations consecutive on the fever, and perhaps is in some degree allied to pyæmia

It seems a subject not unworthy of clinical inquiry, but which I do not think has received investigation, whether the tendency to acute inflammations, as a consequence of rheumatic fever, is not developed principally in those persons who have disobeyed the warning to keep quiet, which the pain caused by motion so feelingly preaches to them Our young patients, who will not be controlled, and our poor patients, who are unwilling or unable to give up work, are certainly those in whom we most commonly find the various degrees of these inflammatory lesions My own experience, limited as it is, of rheumatic fever among the upper ranks, certainly makes me fear pericarditis less in them than in the frequenters of hospitals, which has led me to think that the "*comfortable classes*" are little liable, while life insurers, who may be taken as a type of the "*prudent classes*," when they confess to having had rheumatic fever, scarcely ever have any sure symptom to relate of chest-affection, or any marks to exhibit of having been treated for it The power, then, and the will to keep quiet at the commencement of acute rheumatism, seem to me the great preventives of secondary inflammations

Though it does not come strictly under the present head, I cannot forbear drawing attention to the frequency with which this same tendency (to become locally inflamed in consequence of motion) is exhibited throughout the whole persons of those affected with rheumatic fever The localization of the rheumatic action in one or more joints, and the putting on of sthenic, and then of disorganizing inflammation, occurs by far the most often in those who, through wilfulness, ignorance, or necessity, have made the greatest efforts to war against pain, and keep about in spite of it I believe most practical observers agree with me in this, yet, strange to say, it is scarcely noticed by writers on the subject We find great varieties of treatment recommended, by which we flatter ourselves we somewhat reduce the duration of the distemper, but we never feel quite happy while employing any, for each has, at some time, received the reproach of doing harm while it relieves the immediate pain. The pupils of Sydenham used to feel great comfort in seeing their sick easier after bloodletting, till Dr. Todd told them that they increased thereby the liability to pericarditis; colchicum made both patient and practitioner joyful, till a suspicion was started, that the anæmia which so often occurs in gouty and rheumatic constitutions, was due to the soothing drug. And so on, throughout the list. We use them all, but we use them as two edged tools. Not such is rest: it cannot injure, and I believe does more to prevent the ill consequences of rheumatic fever than any other treatment. One who would spend time in making this humble anodyne and prophylactic generally known, would deserve the gratitude of society. Recommendations of it *ad clerum* are, it may be hoped, superfluous; but unless we inquire, we should hardly guess how wide spread among the laity is the idea, that all pain, especially gouty and rheumatic pain, should be fought against to the utmost, and that rest is a temptation to be resisted.

H. *Remains of former Pericarditis.*

There is this distinction between the lesion now under consideration and those which will follow—viz., that it represents only the former existence of an acute inflammation which has passed away, whereas the morbid conditions of valves to be enumerated next in order are a continually augmenting evil, in all probability, and may fairly be called a chronic disease as well as a chronic lesion.

1. *Adhesions.*—There were 86 cases of *old adhesions* in the pericardium by which the opposed surfaces of the serous membrane were united. Of these 86 internal adhesions 51 were *universal*, completely obliterating the serous sac; 4 *nearly universal*; 29 *partial* bands, of various length; and 2 insufficiently described.

Of the 55 cases of universal and nearly universal adhesion, the *ages and sexes* were known in 54.

The mean age at death of 37 males was 34.6 years.
„ „ „ 17 females was 34.8 years.

Of the 29 cases of partial adhesion, the *ages and sexes* were known in 28.

The mean age at death of 20 males was 42.9 years.
„ „ „ 8 females was 41.5 years.

Deduction.—The consequences of partial adhesion are much less rapidly fatal than of universal adhesion.

It would have hardly been worth while to notice this, had not mechanical grounds been taken for an idea that universal adhesion is the most favourable termination of pericarditis. The above quoted numbers are satisfactory proof that it is not so. They are, indeed, not quite so large as might have been wished, but the similarity of the mean ages of men and women in both classes shows that there exist no disturbing causes of importance to invalidate the correctness of the average.

2 *Incidental consequences of adhesions*—In 5 of the cases with partial adhesions, there was a quantity of *serum in the sac*, varying in amount, but in one case only was it very large

The *mean age* of 4 of those where the age was known, was 43 0 years, so that the chance of fluid in the sac does not add much to the danger

In 5 cases out of the 86 a formation of *bony matter* had taken place in the old fibrin

In 1 case it contained *tubercles*

In 1 case the old adhesions were *œdematous* with serum

3 *Thickening, &c* There are 23 other cases where the pericardium, though not adherent, exhibited traces of *former inflammation*—viz

15, where it was adherent to external parts
6, where it was thickened
2, where there were unattached fibrinous filaments, possibly the remains of bands set loose by absorption of one extremity

4 *White patches on the Pericardium* —Much doubt has, with good reason, been thrown on the assumption patronized by the late Dr Hope, that white patches on the heart are an evidence of former pericarditis However, as that opinion is still entertained by many persons, this would seem the proper place to speak of the lesion

It was found altogether in 160 cases

It was found associated with adhesions of the pericardium but 3 times, in two of which there was fluid contained in the pericardium, and in 1 some recent inflammation With the more ordinary cases of adhesions, it does not seem to have been conjoined

It is, I think, rendered probable from these facts, that though the exudation of fibrin from an acute inflammatory condition, may in some few cases confine itself to a circumscribed place, and form these spots, yet in the great majority of instances they are due to a process different in kind, and affecting different states of system Whether this process is a sort of localized hypertrophy of the membrane, or a union of it with the cellular tissue beneath, so as to become opaque and thick, will receive more evidence from the microscope and the scalpel than from statistics We ought to distinguish more accurately than is the habit at autopsies, whether the opaque patch is capable of removal, leaving the serous membrane beneath, or whether it really forms one continuous structure with it And even then it is probable that the naked eye requires a certain course of instruction by the microscope, before it is able to decide without it on the nature of these membranous structures

I *Chronic diseased states of the Valves*

During the ten years there were 367 cases in which the valves were *chronically diseased*—viz

Thickened or contracted in 156
With morbid deposits in 211

The *particular valves* affected were as follow—viz.:

TABLE IX.—*Thickened or contracted.*

	Times
The mitral and aortic simultaneously	50
The aortic alone	18
The mitral alone	37
The mitral and tricuspid	8
The mitral, aortic, and tricuspid	5
All four sets	4
The tricuspid alone	1
The tricuspid and aortic	2
The aortic and pulmonary	1

TABLE X.—*With chronic morbid deposits.*

The mitral and aortic simultaneously	71
The mitral alone	59
The aortic alone	59
The aortic, mitral, and tricuspid	6
All four sets	5
The mitral and tricuspid	2
The aortic, mitral, and pulmonary	2
The aortic and pulmonary	2
Doubtful	5

That the thickening of the valves above enumerated may be fairly considered under the same head as the chronic deposits of atheroma in various degrees of hardening, of warty growths, and calcareous formations, is assumed from our knowledge of the graduated shades by which these appearances run into one another in individual instances. Another reason for the assumption may be drawn from statistics of the association of these deposits in other parts, with thickening of the valves in the vicinity.

Thus we find *atheroma of the aorta* noticed—

In the 156 cases of thickened valves, 62 times, or in 39.1 per cent.
In the 211 cases of chronic deposits, 90 times, or in 42.6 per cent.

It is possible that where atheroma, or any congenital state of the valve had been noted by the observer, it was in some cases considered superfluous to state also that the aorta was in a like condition; and therefore that the 42.6 per cent. is somewhat under the mark. But still the very frequent conjunction of atheroma of the aorta with thickened valves, is in itself a sufficient argument for their being parts of the same disease, even if we allow that there is a fallacy in the apparent showing of the above table, by which it seems that simply thickened valves are nearly as often associated with atheroma of the aorta as atheromatous valves are.

Deduction.—Thickening of the valves is a lesion arising under similar conditions as chronic deposits of atheroma, earthy matter, &c., in the same parts.

From the above tabular details, it would at the first blush appear, that chronic disease of the *aortic* is at least as frequent as that of the *mitral* valve, and that the assumption drawn from our statistics of acute endocarditis, as to the greater liability of the latter, was erroneous But if we consider the frequency of morbid deposits in the coats of the great vessel as the first commencement of the degeneration, and the likelihood for the neighbouring valves to be thereby affected, we shall see, that for the auriculo-ventricular orifice to be *as often* diseased, shows in it a greater individual aptitude than its colleague suffers from

Thus we find that in the 107 cases where the *aortic* valves were alone diseased, the aorta was atheromatous 55 times, or in 51 4 per cent

In the 96 cases where the *mitral* alone were diseased, the aorta was atheromatous 31 times, or in 32 2 per cent

And of these 31 times, 9 instances occur in the early part of one year, with the signature of a different curator, so I think there is fair reason for suspecting an idiosyncrasy on his part for including the aortic valves under the generic term "aorta," and therefore omitting to notice separately disease of the latter

Deduction —As in acute, so in chronic disease, the most common case is for both sets of valves in the left heart to be affected simultaneously, and of the two, the mitral would appear to have the greater individual liability, though the proximity of the aorta, and its frequent diseased condition, actually put the aortic in most danger

Now if atheroma of the aorta is part of the same disease as thickened and atheromatous valves, it is fair to associate all three together in the view we take of their frequency in the dead body, and in the inferences we thence draw concerning the living I will therefore add to the instances of diseased valves those where the aorta was affected with chronic deposit, without the valves having been partakers in the injury

In the 1794 cases *without* chronic valvular disease, we find the aorta noticed as being in that state 195 times, or in 10 8 per cent

Adding these 195 cases to the 367 of affected valves, we get 562 in the whole 2161, or 26 per cent, as a symbol of the probable frequency of these sister lesions in the sick

Deduction —Chronic degeneration of the valves on neighbouring aorta is at least equally common with tuberculosis, which affects 25 4 per cent.*

Questions of Age and Sex

The tables which follow are designed to illustrate the proportionate frequency with which the lesion in question affects different ages and sexes in those classes of patients which become the subjects of observation at metropolitan hospitals

---

* See Decennium Pathologicum Part I No III, in Medical Times August 14th 1852

TABLE XI.—*Ages and Sexes of those affected with Chronic Degeneration of the Valves and neighbouring Aorta.*

AGES	Thickened valves		Chronic deposit in valves		Chronic deposit in aorta with healthy valves			Total		Total
	Males	Fem	Males	Fem	Males	Fem	Sex unknown	Males	Fem	Both sexes
From birth to 15 inclusive	3	0	1	0	0	1	0	4	1	5
From 15 to 30 inclusive	12	12	23	13	17	14	1	52	39	92
From 30 to 45 inclusive	35	25	41	14	54	13	0	130	52	182
From 45 to 60 inclusive	27	15	50	16	44	24	0	121	55	176
Above 60	12	8	27	15	14	6	0	53	29	82
Age unknown	7	0	7	4	5	1	1	19	5	25
Total of all ages	96	60	149	62	134	59	2	379	181	562
Column	a	b	c	d	e	f	g	h	i	j

TABLE XII.—*Percentage in total Deaths of all Diseases, where an Autopsy was made.*

—	Thickened valves		Chronic deposit in valves		Chronic degeneration of aorta with healthy valves		Total		Total
	Males	Fem	Males	Fem	Males	Fem	Males	Fem	Both sexes
In 154 cases from birth to 15 inclusive—viz. 94 males, 60 females	3.1	0	1.0	0	0	1.6	4.2	1.6	3.2
In 636 cases from 15 to 30 inclusive—viz. 377 males, 259 females	3.1	4.6	6.1	5.0	4.5	5.4	13.7	15.0	14.4
In 651 cases from 30 to 45 inclusive—viz. 472 males, 179 females	7.4	13.9	8.6	7.8	11.4	7.2	27.5	28.6	27.9
In 438 cases from 45 to 60 inclusive—viz. 299 males, 139 females	9.0	10.7	16.7	11.5	14.7	17.2	40.4	39.5	40.0
In 167 cases above 60—viz. 109 males, 58 females	11.0	13.7	24.7	25.8	12.8	10.3	48.9	50.0	49.1
In 111 cases of unknown age—viz. 74 males, 37 females	9.4	0	9.4	10.8	6.7	2.7	25.6	13.5	22.5
In total of 2161 cases—viz. 1425 males, 732 females, 4 ? sex	6.7	8.1	10.4	8.4	9.4	8.0	26.5	24.7	26.0
Column	A	B	C	D	E	F	G	H	I

Deductions

From Columns G, H, and I, in Per-centage Table — Table XII

I The tendency increases with the age

II The rate of the increase is, however, in an inverse ratio to the age

Thus in the cases analyzed it was—

From 15 to 30, as 450 to 100 more likely that a person would be so affected than under 15

From 30 to 45, it was as 191 to 100 more likely than from 15 to 30

From 45 to 60, it was as 144 to 100 more likely than from 30 to 45

Above 60, it was as 123 to 100 more likely than in the preceding fifteen years

The above deductions do not at all lead to the conclusion which is sometimes formed, from the fact that the liability increases with age—viz, that the degeneration is, in truth, *old age itself* Were this the case, the rate of increased liability would be either regular, or would increase in a direct ratio with the years The inference rather is, that it is a disease incidental to development of the body, and from a variety of circumstances, which may be surmised still more incidental to riper life, but by no means a part of the physiological course itself of life

III The liability of males and females differs but little

This may be inferred *from columns G and H*, in which the difference of men and women may be fairly suspected to arise from the narrowness of the field of observation not excluding accidents It is very much under that which was observed in tuberculosis, where the per-centage of males was 27 2, of females, 21 7, a difference entitling us to a decided opinion of a real excess of liability in the former * Such a conclusion would not be legitimate from the data before us, especially as we see that the higher the figures, the more nearly do the proportionate number of the sexes approach one another

But when we come to examine the *earlier columns*, a difference is apparent which seems to demand remark It is to be observed, that up to the age of 60, the cases of thickened valves without irregular deposit (*Columns A and B*) exhibit a decided preponderance on the side of the females, but that the roughening by any of the various grades of morbid formation (*Columns C and D*) is more frequent among males Chronic degeneration of the aorta (*Columns E and F*) without diseased valves shows no variable proportion

IV Though the liability of males and females differ as a whole, yet that is not the case in the different forms, *thickened valves* being oftenest seen among females, chronic deposits among males

May not the greater tendency of males to form these solid matters bear some relation to what was formerly observed about tubercle†—viz, the greater tendency in that sex to its conversion into chalky substance? Has it any connexion with man's tendency to gout and its consequences? It is easier to ask these questions than to answer them.

---

* Decennium Pathologicum Part I No III Medical Times Aug 14th 1852

† Cretaceous matter was found in the pulmonary tissue in 64 cases of whom 45 were males and 19 were females Decennium Pathologicum, Part I No IV Medical Times, Aug 28th 1852

J. *Association of Disease of the Muscular Walls with Disease of the Valves and Aorta.*

The conditions of the muscular walls of the heart, which were found coincident with the previously enumerated cases of diseased valves or aorta, are divided in the ensuing tables.

TABLE XIII.

In the whole 562 cases,

	Times	Per cent
The muscular walls were healthy	214	38.0
,, ,, diseased	348	61.8
In the latter 348 cases, they were—		
Dilated	140	40.2
Hypertrophied	97	27.8
Dilated and hypertrophied	104	29.8
"Enlarged"	7	2.0

TABLE XIV.

In the 367 cases of Chronically Diseased Valves,

	Times	Per cent
The muscular walls were healthy	113	30.8
,, ,, diseased	254	69.1
In the latter 254 cases, they were—		
Dilated	87	34.2
Hypertrophied	74	29.1
Dilated and hypertrophied	89	35.0
"Enlarged"	4	1.5

TABLE XV.

In the 156 cases of Thickened Valves,

	Times	Per cent
The muscular walls were healthy	43	27.5
,, ,, diseased	113	72.4
In the 113 latter cases, they were—		
Dilated	44	38.9
Hypertrophied	34	30.0
Dilated and hypertrophied	33	29.2
"Enlarged"	2	1.7

TABLE XVI.

In the 211 cases with Chronic Deposits in the Valves,

	Times	Per cent
The muscular walls were healthy	70	33.1
,, ,, diseased	141	66.8
In the 141 latter cases, they were—		
Dilated	43	30.4
Hypertrophied	40	28.3
Dilated and hypertrophied	56	39.7
"Enlarged"	2	1.4

Table XVII

In the 195 cases, where the Aorta was hardened, there being no Disease of the Valves,[*]

The muscular walls were healthy	101	51 8
„ „ diseased	94	48 2

In the 94 latter cases, they were—

Dilated	53	56 4
Hypertrophied	23	24 5
Dilated and hypertrophied	15	15 9
"Enlarged"	3	3 2

To compare with these, we find the following 157 cases *where the muscular walls were diseased without any lesion of the valves or aorta—viz*

Table XVIII

	Times	Per cent
Dilated	69	43 9
Hypertrophied	40	25 4
Dilated and hypertrophied	35	22 2
"Enlarged"	13	8 2

Deductions

Hence it appears that—

I *If degeneration exists in the great vessels or valves*—

That the muscular walls will also be diseased is rather more than 3 to 2

If the valves are diseased—

That the muscular walls will also be diseased is rather less than 7 to 3

If the valves are healthy, but the aorta diseased—

That the muscular walls will also be diseased is not quite an even chance

II *In thickening (which we may fairly view as the first stage of degeneration) of the valves*—

Simple dilatation is the most common condition of the muscular walls

Where the disease has advanced to chronic deposits—

Dilatation with hypertrophy is the most common

Where the valves and aorta are healthy, and the muscular walls diseased—

Simple dilatation is (as in thickening of the valves) the most common, but instead of being one-fourth more common, it is nearly twice as common as any of the other states

---

[*] From this and the following tables cases where recent fibrin only was found on the valves are excluded

Where the muscular walls alone are diseased—

The order of frequency of the form of disease is as follows dilatation, hypertrophy, dilatation joined to hypertrophy

But where the valves are diseased (especially in the most advanced state)—

Hypertrophy joined to dilatation is the most usual form of lesion *Simple lesion of the aorta* follows the same rule as disease of the walls alone

Corollary—It is probable, therefore, that valvular disease, by its continuance, tends particularly to generate hypertrophy of the heart, and that other causes more usually produce dilatation

A general view of the comparative *frequency of occurrence of the several lesions mentioned in the foregoing tables* may be obtained from the list which follows

TABLE XIX

	Times	Per cent
Diseased valves, with healthy heart	113	5.2
Diseased valves, with diseased heart	254	11.7
Atheromatous aorta, with healthy valves and healthy heart	101	4.7
Atheromatous aorta, with healthy valves and diseased heart	94	4.3
Diseased heart, without any of the above lesions	159	7.3
Heart free from the chronic lesions, included in the above enumeration	1440	66.7

K *Disease of the Muscular Walls*

A general view of the frequency of occurrence of the different forms of diseased muscular walls is given in the following table

TABLE XX

Form of Cardiac Disease	Valves chronically diseased	Aorta diseased no disease of valves	Both healthy	Total
Dilatation	87	53	69	209
Hypertrophy and dilatation	89	15	35	139
Hypertrophy	74	23	40	137
"Enlargement" (form not specified)	4	3	13	20
Total	254	94	157	505

The openings for fallacy in the foregoing table are the following

In some cases of diseased valves, "hypertrophy" may have been intended to include "dilatation" also

Some few cases of tonic contraction of the ventricles may be included under "hypertrophy, with valves and aorta healthy"

How many times this may have happened it is impossible to estimate, but the general character of the curators is a guarantee that it was not often, and the following conclusions are probably not thereby affected

As a general rule, the most common diseased condition of the muscular walls is dilatation

With diseased valves, dilatation with or without corresponding hypertrophy, is about equally common

With diseased aorta, dilatation alone is most common

Where both valves and aorta are healthy, simple dilatation is *by far* the most common

L *Association of Emphysema of the Lungs and Diseases of the Heart*

TABLE XXI

	Altered muscular walls without valvular or aortic disease	Altered muscular walls with chronic valvular disease	Altered muscular walls with atheroma of aorta but no chronic valvular disease	Chronic valvular disease without alteration of muscular walls	Atheroma of aorta, without alteration of muscular walls
In 258 cases with emphysema of lungs	30	58	15	18	19
Per cent	11·6	29·1	5·8	6·9	7·3
In 1903 cases without emphysema of lungs	127	196	79	95	75
Per cent	6·6	10·2	4·1	4·9	3·9

From this table we may make the following deductions

I Emphysema has a very marked tendency to be connected with all forms of disease in the heart, or in its valves and vessels

II The tendency is most marked in cases where the valves are diseased, as well as the muscular walls

It may be seen that the per centage of emphysematous cases bears to the non emphysematous cases a ratio of 219 to 100 where the valves are dis cased with the muscle—a ratio of 175 to 100 where the muscle alone is diseased.

III. Valvular disease alone, and aortic disease alone, have some tendency to be connected with emphysema, and the latter more than the former.

The last clause, perhaps, is in a great measure due to most of the aneurisms and marked dilatations of the aorta being included in the atheromatous cases.

It does not seem likely that emphysema should produce valvular disease, and therefore—

IV. Where cardiac disease is associated with emphysema, it is most likely that the former is the primary lesion.

The subjoined table (XXII.) shows that—

V. There is no very marked difference between simple thickening and chronic morbid deposit in the valves, as respects the above lesion, but it is *most* marked in the latter.

TABLE XXII.

	Altered muscular walls with simple thickening of valves.	Altered muscular walls with chronic deposit in valves.
In 258 cases with emphysema of lungs	26	32
Per cent.	10.0	12.1
In 1903 cases without emphysema of lungs	87	109
Per cent.	4.5	5.7

XI. *Association of Pulmonary Tubercle and Disease of the Heart.*

In the first part of the 'Decennium Pathologicum,' No. VII., published in the 'Medical Times and Gazette' of November 6th, 1852, I have at some length discussed the question propounded in this section. It is unnecessary, therefore, to repeat the statistics there given. The conclusions drawn are—

I. That cardiac disease is less usual in cases of tuberculosis than in mixed cases.

II. The inclination of the evidence is to the view that cardiac disease does not so much render tuberculosis improbable, as tuberculosis renders cardiac disease improbable.

It may be mentioned that the latter conclusion is in opposition to the conjectural statement of Professor Rokitansky, that disease of the heart confers an eminent immunity on its possessor against tuberculosis.

N *Association of Hepatization of the Lungs and Disease of the Heart*

I do not feel sufficiently assured that throughout the ten years accurate distinctions have been made between acute pneumonia and chronic atrophy of the pulmonary tissue, to make it safe to draw any statistics from the post-mortem records on this point *A priori*, conjecture would lead us to expect that the former was likely to be most usually a consequence, and the latter a cause, of cardiac dilatation or hypertrophy

The following comparison is probably only an approximation to truth, and gives little information

In 505 cases of *diseased heart*, hepatization of the lungs occurred 96 times, or 19 0 per cent

In 517 cases of *tubercle of the lungs*, it occurred 218 times, or 42 1 per cent

In 1139 *other cases*, 231 times, or 20 2 per cent

O *Association of Renal Degeneration with Heart-Disease*

This has been fully discussed in the April number of this journal (Decennium Pathologicum, Part II), and the remarkable frequency with which it appears to be the cause of the cardiac lesion is there exhibited

ART III

The Internal Surface of the Uterus after Delivery, its Analogues, &c By J MATTHEWS DUNCAN, A M , F R C P E , Lecturer on Midwifery, Edinburgh, &c &c

THE internal surface of the uterus after delivery may be divided into three principal parts 1 The inner surface of the cervix , 2 The site of the insertion of the placenta upon the inner surface of the body of the uterus , and 3 The rest of the inner surface of the body of the organ

The first of these parts is entirely excluded from discussion in the present paper The cervix of the uterus is now known to differ so materially in its anatomy and physiology from its body that it may with propriety be described as almost a separate organ The researches of M Stoltz have been confirmed by numerous authors, and seem to show that the cervix uteri does not become developed so as to form part of the cavity of the organ in the way that it was formerly believed to do More recent researches by anatomists and obstetricians, too numerous to be mentioned, have shown the entire difference, anatomically and structurally, as well as functionally, of the mucous membrane of these two parts And, in more immediate relation to our present subject, it may be stated, that there is little variance of opinion as to the inner surface of the cervix being provided with a mucous membrane after as well as before parturition

The other two parts of the internal surface of the uterus after delivery form the subjects of our present communication and I may here be permitted to state, that my object in this paper is to attempt to point out correct doctrines in regard to this most important subject With this I shall content myself at present, leaving the numerous practical deductions from it for another occasion.

Every attentive student of obstetrics must have been struck with the manifest paradoxes implied in the history given of the termination of parturition, and the restoration of the inner surface of the uterus to its pristine condition. Obstetricians now teach that with the expelled ovum there passes away the entire decidua, leaving the muscular tissue of the uterus bare over its whole inner surface; and, in contradiction at once to observation and analogy, describe the formation over this surface of a false membrane, and afterwards of a new mucous membrane—all this process being, even in the healthy female, an inflammatory one, and, like superficial inflammations in such structures, accompanied by the secretion of a quantity of pus which they describe as constituting the essential part of the lochia. To adduce evidence of this statement here is, perhaps, not necessary, as the description must be readily recognised by almost every one; and in a subsequent part of this paper I shall return to these errors, and dwell at some length upon them; at present I may merely remark, that it appears wonderful that so many physiological paradoxes should have been so easily adopted by the profession, and that so much of what is essentially morbid should have been invoked to aid in the performance of a natural function in a healthy body.

In the early months of pregnancy, the mucous membrane of the cavity of the uterus is very highly developed, forming a rich, soft lining to its inner surface, and contributing greatly to the increased thickness of the parietes of the organ at this time. When the walls of the uterus are examined in advanced pregnancy, the thickness of this structure is found to be greatly diminished, even when the whole structures between the chorion and the muscular tissue of the organ—that is, the proper mucous membrane, or decidua vera, and in addition, the decidua reflexa—are included in the measurement. If, in a woman who has died in advanced pregnancy, the membranes are separated from the uterus, the mucous membrane is left adhering to the walls of the womb, only its surface is lacerated and irregular.* I have witnessed the hurried post-mortem examination of a woman dying during labour and before rupture of the membranes, where the ovum was almost completely separated by hæmorrhage extravasated between the membranes and uterus. In this case the uterine fibres did not appear to be anywhere denuded; but the examination was not sufficiently minute and satisfactory.

In parturition a similar process takes place, with this difference, that after the removal or expulsion of the ovum, the uterus is reduced by its contractions to dimensions very small compared to those it had whilst expanded. The result of this contraction, upon the mucous membrane, resembles its effect on the muscular tissue of the organ. In both, the diminution of superficial extent is the result partly of the expulsion of the large mass of blood contained in their very large vessels, but chiefly of the assumption of a greatly increased thickness of wall. After parturition, the mucous membrane of the uterus is rough and irregular on the surface, and covered over with blood and adherent coagula.* As the uterus diminishes in size, its thickness increases. In a preparation in my possession, taken from a woman who died soon after delivery, where the os uteri is still largely dilated, and the utero-placental veins open, the length of the uterus is about seven or eight inches, its thickness, including all its parts, is less than one inch. Its internal surface is covered by a thin membrane. This latter is thicker and more prominent at the seat of the placental insertion. Judging from the extent of surface occupied by open-mouthed veins, the placental site is reduced to a circular or rather oblong space, of about three inches in diameter. In another preparation, the uterus is of about the same size, above an inch in thickness, the site of the placenta is evident from the prominence and softness of the part, the uterine veins not prominent and gaping, but apparently closed, and the mucous lining thicker and soft. This uterus is evidently that of a woman who has died some time after delivery. In another case, where the mother died on the fourth day after delivery, I had through the kindness of Dr W. T. Gairdner, an opportunity of examining the uterus. It measured about seven inches in its greatest length, and the thickness of its walls was about three quarters of an inch. The whole inner surface of the organ was manifestly covered by a mucous membrane, lacerated at the site of the placental insertion, a surface of between three and four inches in diameter, a number of clots of blood were entangled in the venous openings. Elsewhere, the mucous membrane was distinct. It was covered by the lochial secretion. On scraping the surface, the lochia and epithelium were easily removed, laying bare the fibrous structures of the mucous membrane beneath. In another preparation, which I examined lately with Dr W. T. Gairdner, which was procured from a woman who, although engaged nursing a child, denied having been recently delivered, and where the woman died of phthisis, we found the lining membrane of the uterus very thick, forming nearly one-third of the entire thickness of the walls of the organ, but thinning rapidly as it approached the cervix. The site of placental insertion was well marked by the prominence of the lining membrane, by its numerous elevations and depressions, covered with serous fluid, and by the large veins which were [illegible] seen in [illegible] section, proceeding through the mucous tissue to the very surface of the membrane,—making it evident that the mucous membrane is not developed as a new production over the venous orifices. Between the placental insertion and the cervix, a punctuated appearance, produced apparently by the openings of the follicles of the membrane was seen In this case the uterus measured about five inches in length, and the woman had passed the period of confinement at least four weeks In the uterus of a woman dying the day after a difficult delivery, I found its inner surface covered by a copious soft membrane, the inner surface of which was very dark in colour, covered with blood, and almost gangrenous in appearance The woman died from peritonitis, the result of hæmorrhage into the peritoneal cavity from a rupture In addition, I may state, that all authors, even those who assert that the muscular fibres of the uterus are denuded after delivery, as Cruveilhier, Fergusson, and others, yet describe their dissections as displaying an inner or lining membrane covering the inside of the uterus *

---

* These facts I have verified by means of several dissections preserved in the late Dr. Campbell's museum. Albinus in his Atlas of Anatomical Plates describes the same results of a similar dissection. See Uteri Gravidi Tab. VII. His words of description are: Ovum exemtum. Uteri pars interior mollis, tenera, veluti spongiosa cavernosaque, cui ovum molliter adhæret involucro suo membranaceo. See also W. Hunter, Anatomical Description of the Gravid Uterus, Ed. 1843. With his admirable accuracy, W. Hunter describes (p. 47) the adhesion of the decidua to the muscular fibres of the uterus as being rather stronger than the adhesion between its external (decidua vera) and internal stratum (decidua reflexa); which we may presume is the reason that in labour it so commonly leaves a stratum upon the inside of the uterus. And he elsewhere makes the general statement that one stratum of the decidua is always left upon the uterus after delivery. See also J. F. Meckel, Descriptive Anatomy (Eng. Trans.), vol. ii. p. 598.

---

* In his Researches on the most important Diseases of Women, p. 36, Dr Robert Lee thus describes the interior of the uterus after labour in the healthy state. For several days after delivery, where no disease of the uterus has supervened, its lining membrane is coated with a yellowish brown, dark red, or ash grey coloured layer, of no great thickness, which seems to be formed chiefly of the fibrine of the blood, with small portions of deciduous membrane. Where the placenta had adhered, numerous dark coloured coagula of blood are found to seal up the orifices of the uterine sinuses in the inner membrane, and frequently to extend a considerable distance into these veins. The clots of blood, one extremity of which hangs loose within the cavity of the uterus, are often connected with a large fibrinous coagulum, which entirely fills the fundus uteri, and everywhere firmly adheres to the inner surface of the organ.

It is, then, found, that after delivery the muscular fibres of the uterus are not laid bare, but are covered by a mucous membrane This membrane is undoubtedly the remains of the uterine decidua, a mucous structure having the peculiar characters of the uterine mucous membrane When examined at this time, it is found, as in early pregnancy, to be thicker at the site of the insertion of the placenta than elsewhere In that site its surface is diversified by numerous elevations and depressions, and by the open mouths of the uterine veins, which have been, as it were, cut across by the separation of the placental mass

The cotyledonary placenta in the living cow, and in many other species of quadrupeds, is formed, like the human placenta, of an umbilical or fœtal portion, and an uterine or maternal portion But the process of separation of the placenta in parturition is conducted very differently in these two animals In the cow, the placental mass is not entirely separated from the uterus The fœtal part alone is discharged with the membranes, the maternal part is left attached to the mucous membrane of the uterus, of which it is merely a part highly developed for a special object There is no wound left upon the surface of the uterus of the cow, no vessels are divided in the process But although there is no wound, yet the cotyledonary surfaces are left in a denuded condition, these parts not being covered by the ordinary epithelial structures In woman, the separation of the placenta is very different The after-birth of the human female consists of two parts—a fœtal and a maternal portion The essential part of the maternal portion is the development of the inner tunic of the uterine vessels, which is prolonged into the after-birth, and springs from the numerous vascular canals which traverse the decidua at the site of the insertion of the placenta, and which are cut across, as it were, by the separation of the placenta The after-birth of woman thus contains, besides the fœtal structures, the important maternal venous structure just mentioned, and also the layer of decidua which covers its surface, and is described as throwing processes into its substance The separation of the placenta thus involves the cleaving of the mucous or decidual structure at the line of separation, and the section of the serous internal tunic of the vascular system of the mother at numerous points And when we con-

---

* See the cases recorded in Livraison xiii of Cruveilhier s Atlas of Pathological Anatomy; also the cases in Dr Fergusson s Essay on Puerperal Fever

sider the extremely soft and fragile nature of both these structures, we shall not be astonished at the great facility of separating the placenta.[*]

After the separation of the after birth the internal surface of the uterus at its former site is left covered by a thicker layer of the decidua than elsewhere The internal surface is at this part more rough and irregular, and more prominent, than elsewhere These vascular openings are numerous, and spread over a surface which is, from the contraction of the entire organ, much smaller than the original surface occupied by the placenta

But, even in woman, the anatomical imitation of the process, as it takes place in the cow, is not impossible W Hunter succeeded in performing this operation in a conception of four months At this time, he says, the union of the two constituent portions of the placenta is less intimate, and they may both be preserved very entire, like the vascular chorion and fungus in the quadruped [†]

The membrane is easily distinguished from the muscular tissue of the uterus by its softness, and by difference of colour in a cross section It is in some cases so soft that its surface can with facility be almost completely rubbed off or brushed off the subjacent tissue And if this manipulation be practised upon it, the investigator will assuredly find no difficulty in discovering the fibrous tissue to be bare, like the muscles in an amputated stump This softness and friability is undoubtedly one of its characters which has given rise to the erroneous opinions of authors, for we frequently find them speaking of removing a soft membranous or flaky structure, in order, as they imagined, to display the real internal surface of the womb For example, W Hunter, in describing[‡] a dissection of the uterus of a woman who died at the end of the ninth month without being in labour, states that, finding the internal surface of the uterus everywhere covered with a thin stratum of decidua he rubbed off the tender membrane with a cloth, in order to expose the subjacent muscular structure But numerous later investigators have not so correctly appreciated, as W Hunter did, the nature of the structure they removed in a like manner, and which they believed to be effused lymph, false membrane, bloody coagula, or patches of decidua having no necessary existence there, and ready to be discharged or rubbed off in order to expose the muscular fibres, which they erroneously believed came to be exposed after parturition We thus find that here, as elsewhere,[§] W Hunter's accounts of his dissections are still in our day true in almost every particular But whilst there can be no doubt of the truth of W Hunter's anatomical description of this part, exception may very justly be taken to the opinion he expresses as to this residuary decidua He states his belief that "most of it dissolves, and comes away with the lochia "[*] Now there is every probability that this takes place in a very different way. The residuary decidua forms the mucous lining of the uterus, and may pass away from the uterus, not in mass, but in the regular insensible exfoliation of such structures, or be removed by the vessels in its substance in the ordinary course of nutrition and absorption

---

[*] "The separation " says W Hunter of the placenta from the uterus is commonly practicable with the least imaginable force —Anatomical Description of the Human Gravid Uterus, Ed 1843 p 37

[†] In regard to this dissection Hunter adds I wished to give a figure of it but the processes were so irregular and so changeable while floating in the water that the painter could not express them, and when taken out of the water they collapsed into a smooth membranous appearance "—Anatomical Description of the Human Gravid Uterus 1843 p 36

[‡] Ibid p 26 In his paper on the Muscularity of the Uterus Sir C Bell writes thus "Upon inverting the uterus and brushing off the decidua the muscular structure is very distinctly seen "—London Medico Chirurgical Transactions vol iv p 341

[§] See in regard to the decidua in early pregnancy Monthly Journal of Medical Sciences for April, 1853, p 326

Moreover, when we inquire into the real nature of the discharges from the uterus after delivery, we shall find that they are of a nature and character quite antagonistic to the notions entertained as to the denudation of the muscular fibres, and the formation of a new mucous membrane after the deposition of a false membrane over the supposed wound, and under the influence of an inflammatory process No doubt, the authors who hold these views state that, in accordance with them, the lochia is essentially a puriform fluid, at first mixed with blood, and afterwards giving place to a serous discharge,[†] but it is impossible to understand whence these authors have derived their information as to the purulent discharge which they describe the lochia to be

To the most ordinary observer, the lochia in the healthy female always presents an appearance far removed from that of purulent discharge from an extensive superficial wound, like that described to exist in the uterus at this time The lochia have been frequently and correctly described as presenting three different appearances in correspondence with three different stages in the condition of the internal uterine surface after delivery These have received the names of *lochia cruenta, lochia serosa,* and *lochia alba, vel mucosa, vel lactea,*[‡] this last is sometimes also called *purulenta,* but this more from theoretical notions in regard to it than from any resemblance it shows to purulent discharge No doubt, pus may often be observed in greater or less abundance in the lochia mucosa, proceeding probably from patches of inflamed surface on the uterus or vagina, or from healing lacerations, but purulent discharge is not found in any quantity, if at all, in the healthy lochiæ After the blood has disappeared from the lochia, it is generally observed to be a more or less clear viscid fluid, of a whitish, brownish, or yellowish colour, and wanting the rich yellow colour, creamy consistence and appearance of pus discharged from a healthy wound Under the microscope[§] it presents blood-corpuscles, entire or breaking-up, epithelial cells, of various forms, and abundant detritus of the same, and along with these, some mucous globules When pus becomes mixed with the lochia, as is not unfrequently the case, it is discovered by its different appearance and characters

Of late years,[||] it has been customary to compare the internal surface of the body of the uterus after delivery to a great wound, or solution of continuity This fallacious analogy was in modern times originated by Van

---

[*] Loc cit p 47

[†] ' Ordinarily says Cruveilhier this false membrane is thrown off with a purulent discharge, which is the lochia '—See Fergusson loc cit See also Velpeau Traité des Acc Ed Bruxelles, p 518 Litzmann Aussatz uber Schwangerschaft Wagner s Handworterbuch iii 2 § 145

[‡] See Maygrier L Art des Accouch tom ii p 218 Boivin Mem de l Art des Accouch p 446 Chailly Traité Prat des Acc 1853 p 395 Jacquemier Manuel des Acc tom ii p 584 Churchill, Theory &c of Midwifery second edition, p 188 Naegele Lehrbuch der Geb fur Heb p 186, Scanzoni Lehrbuch der Geb p 267

[§] See Gruby Morphologia pathologica, p 20 for some remarks on this subject

[||] Rigby, System of Midwifery p 269

Swieten, but it is due to him to state, that his notions on this subject were, in many respects, more true and correct than those of Cruveilhier and his followers in our own day Thus, Van Swieten points out,[*] that after the separation of the chorion and placenta, the inside of the uterus is left covered with the remains of the *tunica cellulosa,* or *substantia cellulosa,* which separates the chorion from the proper tissue of the uterus He does not describe the uterine muscular fibres as being laid bare, but states that, in addition to the rupture of numerous large vessels, there is[†]—in his own words—*soluta cohæsio, recens, cruenta, partis mollis, id est vulnus,* and then he goes on to point out the particulars in which this solution of continuity differs from an ordinary wound In a subsequent paragraph he states his belief that the fever commonly called *febris lactea* does not arise from the afflux of milk to the mammæ so much as from the superficial suppuration which he describes as taking place on the internal uterine surfaces [‡] In his excellent treatise on child bed fevers, Dr Kirkland[§] takes opportunity to corroborate the statements of Van Swieten on this subject

M Cruveilhier is the anatomist of our own times who has been most explicit in this matter, and it is to his misstatements that are due many of the anatomical and pathological errors in regard to it which have found currency among distinguished teachers and authors in our own and other countries Cruveilhier's views are detailed in his great work on pathological anatomy,[||] and his words are quoted with approval by Drs Fergusson,[¶] Rigby,[**] and numerous others

In the passage alluded to, one of the first statements, in regard to the inner surface of the uterus, is to the effect that the "whole of the mucous membrane has been altered by the inflammation of which it has been the seat "[††] It is impossible to reconcile this quotation with what follows it almost immediately, to the effect that "except just at the inner surface of the cervix uteri, there is no mucous membrane at all, but the muscular tissue of the uterus is everywhere exposed "[‡‡] It is quite needless to attempt to make these statements tally, seeing that in the act of parturition no inflammatory process goes on whatever Cruveilhier's assertion is totally unsupported by observation, and is quite inconsistent with the general principles of physiology, which do not admit of any morbid process, such as inflammation, acting a primary or subordinate part in any natural and healthy function In natural parturition, as in all other healthy actions of the body, inflammation interpolates itself only as a cause of derangement or destruction of the function Not very long ago, the history of the origin of the decidua, or modified mucous lining of the uterus, was involved in obscurity and John Hunter, in attempting a theory for it, founded on erroneous and imperfect observations, was driven to suppose that in healthy pregnancy a sort of inflammation was set up on the internal surface of the uterus, producing there a layer of coagulable lymph, which he imagined came to form the decidua Into a similar error M Cruveilhier and his followers have fallen There is no evidence whatever of the existence of inflammation of the internal surface of the uterus in natural delivery The only process like inflammation in this part is at the seat of the insertion of the placenta, where the closure of the disrupted vessels may be said to take place by a sort of inflammatory process *

---

[*] Commentaria in Aphorismos &c Lugduni 1764 tom iv p 604, [1329] Cum ergo uteri superficies lacerae tunicae illius cellulosae reliquias habet '

[†] Ibid p 604

[‡] Ibid, p 605 *Videtur hinc valde probabile esse febriculam illam quæ in puerperis lactea solet dici non tantum a lacte ad mammas delato nasci sed etiam a depuratione uteri per blandam talem, et superficiariam suppurationem* '

[§] A Treatise on Childbed Fevers and on the Method of Preventing them &c &c, 1774, p 75

[||] Livraison xiii [¶] Essays on the Most Important Diseases of Women, part 1, p 77

[**] System of Midwifery p 270

[††] Essays on the Diseases of Women by Dr Fergusson p 77 The words in the original are—' *La muqueuse intérieure n'existe plus ou plutot ses éléments se sont dissociés, modifiés par l'inflammation dont elle a été le siége* '

[‡‡] Ibid p 77 The words in the original are—"*On ne trouve de debris de muqueuse que sur la face interne du col uterin et quelquefois autour de l orifice des trompes, partout ailleurs le tissu propre de l uterus est à nu et partout il doit être recouvert d une cicatrice*" See also Heschl, on the Human Uterus after Delivery 1853 p 6

M Cruveilhier goes on to state, that after parturition "the muscular fibres of the uterus are everywhere exposed "† But of this he gives no evidence whatever On the contrary, the dissections he describes, although they are morbid, give some evidence to the contrary effect In fact, the examination of this part after death overthrows this statement of Cruveilhier, Fergusson, and others The actual observations of these authors form a curious commentary upon their expressed opinions It is unnecessary to refer to and quote these observations, where the inner membrane of the uterus covering its muscular fibres is incidentally mentioned or described at length

Finally, did there exist after every delivery a wound of the enormous dimensions of the internal surface of the uterus—dimensions not inferior to those of the wound produced in amputation of the thigh, it is difficult to conceive how parturient females should escape the frightful mortality succeeding that operation, or the like It would be difficult or impossible to explain why, instead of one in every three or four dying as after ampu-

---

* It would be wrong to admit the process of closure of these vessels to be an inflammatory or morbid process The closure of the foramen ovale in the fœtal heart by adhesion or cohesion of the valve to the opposed border of the opening does not involve an inflammation of the inter auricular septum or of any of its parts The inflammation of the inner surface of the uterus as described by Cruveilhier is undoubtedly a very different process and undoubtedly a morbid one, resulting as he says it does in the formation of pus and of false membrane

† In an interesting paper on the Dysmenorrhœal Membrane published in the Monthly Journal of Medical Science for Sept 1846 Dr Simpson states his belief that the actual absence of the mucous surface of the uterus has been often ascertained on dissection and adds I lately saw a case where the patient died six weeks after delivery and still at that late date after confinement the mucous lining of the uterus was not yet regenerated ' This case is not given at sufficient length to allow of any judgment in regard to it and as it stands it is quite unsatisfactory Further, Dr Simpson states, that the absence of the mucous lining of the uterus in persons who have died after delivery or who have been previously subject to membranous dysmenorrhœa may have given rise to the strong opinions expressed in regard to the human uterus not being normally provided with a mucous membrane But it has yet to be shown that at any time the uterus is denuded of mucous membrane And certainly there is no reason to believe that as Dr Simpson states, the proper mucous tissue of the uterus itself may within the compass of a menstrual period form enlarge separate and again be reproduced and further that all this may occur and continue regularly for a succession of months or as sometimes happens for a succession of years The discharge of the dysmenorrhœal membrane (or as it may be called inflamed menstrual decidua) does not involve the removal of any part of the entire thickness of the mucous membrane A comparison will illustrate my meaning The cuticle is in the healthy state constantly and insensibly exfoliating in dusty particles In like manner the mucous membrane of the uterine cavity undergoes an insensible exfoliation But let the skin be inflamed as by a blister and then the cuticle is detached in mass and separated as a membrane the proper tissue of the skin is however not removed In like manner, when the mucous membrane of the uterine cavity is inflamed as in dysmenorrhœa, the thick and rich epithelium is detached in mass and separated as a membrane , the proper mucous tissue of the cavity of the uterus is, however, not necessarily removed

tation of the thigh, there should be only one in every two or three hundred It can scarcely be asserted that the shock produced, and the circumstances of the supposed uterine wound, are a whit more favourable to recovery in the obstetric than in the surgical patient The explanation lies in the fact that the chief analogy of the internal uterine surface after delivery, is not with a stump, so far as it consists of incised and denuded tissues, but only in both surfaces presenting numerous open veins liable to become inflamed, or to absorb the obnoxious materials which may be brought into contact with them And it is to this anatomical circumstance that are traceable most of the cases of death in childbed

---

Art IV

Historic Data, &c, in reference to some points of Infantile Pathology By W Hughes Willshire, M D Edinburgh, senior Physician to the Royal Infirmary for Children, &c *

No I — Pneumonia

Notwithstanding the attention which has been paid in this country, during late years, by many practitioners to the peculiarities and effects of diseased action in the child, and after making full allowance for the extension of sound and advanced opinions since the publication of the lectures of Dr Charles West, yet it cannot be denied that there are still many (some of whom are well acquainted with the pathology of the adult frame) who are unaware of the litigated and unsettled position which certain questions respecting the pathology of children have occupied, and even occupy at the present time Were such questions relative only to rare or unfrequent diseases, or, though connected with affections of common occurrence, disputed merely by men of limited knowledge or experience, the author would not think it advisable that they should be discussed in these pages, but inasmuch as some of them refer to things of every day frequency, and are vigorously combated by very high authorities, he has deemed it might not be out of place to draw the attention of such to them, who, though not desiring to enter deeply into the minutiæ of recondite points in *pædiatrics*, may yet feel interested in some important features of common forms of disease As the pages of a serial are often read when those of a systematic treatise are neglected, it may happen that the following analytic sketches (which are all that can be attempted by the author) shall be an inducement to some to seek in such treatises a more extended account of the subjects therein touched upon It is intended that pneumonia, *in some of its bearings*, be first illustrated, but only, it must be remembered, in a very general way, the landmarks are pointed out which may indicate the path to those who wish to explore it more minutely, in the words of Orfila, "Il est toujours utile d'essayer de frayer la route quand même elle sérait imparfaitement tracée"

Whatever weight may be attached to the knowledge presumably possessed by the more ancient writers, or to that indicated by the allusions of

---

* It is proper to state that nearly the whole of this paper was in our hands before the article on Bronchitis Pulmonary Collapse, and Emphysema in our April number, was received.—Editor

Stoll, Sydenham, Morton, Rosen, &c, concerning a general idea of inflammation of the respiratory organs in children, and as leading them to make a distinct separation between the affection attacking the upper air-passages and the parts within the thorax, it may be affirmed, that it was not until the time of Reil, in 1792, that distinct proof was given that the differential lesional signs of bronchitis and pneumonia had been recognised This we may assume to have been done, from the account given in the 'Memorabilia Clinica' of the appearances found after death in some of those who died during an epidemic of small-pox, raging at Halle in 1791[*] The redness of the mucous membrane, even in the fine ramifications of the bronchia, was noted, and also a change in the parenchyma of the lung, which was said to be due to inflammation of the latter For more than twenty years from this period there seems to have been a strong tendency to neglect the study and more perfect elucidation of the differential signs of the two diseases, in particular, losing sight of pneumonia, or merging it as much as possible in bronchitis, the history of which, since its establishment by Reil, was progressively advancing This fact the writings of Thomann, Cheyne, Cuming, Badham, Kersig, and others, sufficiently testify, in which the terms bronchitis, peripneumonia, paralytic peripneumonia, and asthma paralyticum, are employed as designative of the affection of which they treat It will be found, on reference to Meissner's 'Grundlage,'[†] that in 1820 an inaugural dissertation was published in Berlin, "de pneumonia infantum," but what this dissertation especially defines as pneumonia, or what demarcation is established between the disease of the bronchia and of the pulmonary parenchyma, we are, unfortunately, unable to say Under these circumstances, we cannot dispute the claim made by MM Rilliet and Barthez in favour of M Leger,[‡] as having been the first to describe—in 1823—as a distinct affection, the pneumonia of children, both in acute and chronic forms of the malady M Leger was soon followed (1825) by M Lanoix,[§] and the latter by Berton,[||] Bergeron,[¶] Burnet,[**] De la Berge[††] Seiffert,[‡‡] and others, by whom not only were the distinctions between bronchitis and pneumonia affirmed as established, but the essential lesional changes characterizing the latter asserted to be so satisfactorily demonstrated, that it could be laid down not only that pneumonia was a common disease in children, but that as it attacked them it was different from the pneumonia invading adults The nature of this difference, and the special lesions attendant upon the disease in children, were attempted to be formally defined Not alone, however, was an advance thus made, but steps were being taken for distinguishing between the effects of pneumonic inflammation, and particular conditions met with in the lungs of new-born children and of children at the breast, and also for establishing a difference between the pneumonia of the latter and the pulmonic inflammation of more advanced childhood In illustration of the above remarks, the labours of

---

[*] Memorabilia clinica Medico practica Vol ii fasc 1 Fieber lehre Halle, 1792

[†] Grundlage der Literatur der Padiatrik &c &c Leipz 1830, p 93

[‡] Essaie sur la Pneumonie des Enfants Paris 1823 Thèse

[§] Pneumonie des Enfants comparée à celle des Vieillards Paris 1825 Thèse

[||] Sur la Pneumonie Lobulaire ou Partielle Paris 1828 Thèse

[¶] La Peripneumonie des Enfants Paris 1828 Thèse

[**] Journal Hebdomadaire, &c 1833 t xii pp 129—197

[††] Journal Hebdomadaire 1834 No 26 (Schmidt's Jahrbuch, Band v)

[‡‡] Die bronchio pneumonie der Neugebornen und Sanglinge Berlin, 1837

Billard, Cruveilhier, Gerhard,* Rufz, Iorg, Valleix, &c, may be referred to But we now arrive at a time when many workers were in the field, and literature upon the subject so prolific, that it becomes difficult to allot to their originators many observations which were becoming common property In 1838—40, MM Rilliet and Barthez appeared before the profession, but before carrying on the present department it is necessary to draw attention to the following observations in connexion with the general anatomic lesions of the pneumonia of adults

> Leaving out of consideration the more exceptional forms of this disorder, whose symptoms and lesional changes undergo more or less modification by the dyscrasic and other states of the patient's system, it may be borne in mind that the morbid anatomic alterations generally characterizing the primary or idiopathic pneumonia of adult life are, in broad terms, the following More or less of a continuous extent, or lobar portion of the lung, is of a dull or brownish red colour, varying in tone in different parts, and giving rise in a variable degree to a somewhat marbled appearance of the pulmonary parenchyma The latter when pressed wants the springiness of health, and much of its natural crepitation, and when cut, a frothy semi-sanguineous fluid quickly exudes from it If the disease has progressed to its second stage, the diseased portion becomes condensed throughout, crepitation is quite wanting, the colour somewhat lighter than before, the part much softened, and presenting more or less of a granular appearance when a section is made, and which combination of characters constitutes the stage of "hepatization," or "red softening" In a still more advanced degree of the disorder, the hepatization is the same, but the softening is greater, the colour of the lung of a rather dirty light grey or yellowish tint, according as this stage—that of purulent infiltration, or grey hepatization—has extended In whatever state or stage the pneumonic lesions are found, it will be remembered that their course is, under ordinary circumstances, from below upwards, invading more or less of an entire lobe of the lung, and not attacking it in circumscribed, discrete, definite, or scattered patches The second stage may commence centrally or circumscribedly in the parts invaded by the first stage, but the latter itself does not commence so in healthy parenchyma—on the contrary, it is diffusedly continuous †

We now revert to the position which the subject of pneumonia in children was attaining to at the time of the appearance of MM Rilliet and Barthez, drawing attention to the fact of the observations and investigations supporting this position having been made and carried on for the most part in the large foundling hospitals of the Continent It was observed (Gerhard, Billard, Guersent, &c &c) that whilst certain cases of pneumonia presented the same lesional characters as attend the disease in the adult, the same engorgement, red and grey hepatization, the same progress from below upwards, and involvement of more or less of the whole lobe of the lung—in others, and by far the majority of cases, the cadaveric appearances were very different The disease appeared to be of an isolated or discrete form instead of attacking a continuous extent or lobar portion, it invaded the lung in disseminated circumscribed spots, or *lobules*, which were surrounded by parenchyma apparently healthy, and the "three stages" in general still preserved this isolated character, this *lobular* appearance, although in some instances the more perfect demarcation of the latter was lost, in consequence of the affected lobules becoming coalescent. In many of these latter cases such coalescence offered no hindrance to the detection of the primary discrete nature of the disorder, whilst in others it was so intense, or perfect, as to cause a simulation of the *lobar* pneumonia of adults, and for which indeed it was frequently mistaken. Hence, in general terms, we may say that two forms of the affection were now recognised as attacking children—a *lobar* and a *lobular* form; but that "lobular pneumonia" was their pneumonia, *par excellence*. Whilst this position was being wrought out and established, certain other doctrines were being evolved, which it is essential we should now notice.

---

* American Journal of Medical Sciences Philadelphia 1834

† Hasse's Pathological Anatomy Organs of Circulation and Respiration

In 1811, Schenk reported a case in 'Hufeland's Journal,' in which a particular condition of the lungs of a child dying four days after birth was remarked upon. The lungs appeared never to have been perfectly inflated, were condensed, and sank in water; they admitted of artificial inflation, however, and then seemed to be quite healthy. According to Mauch,* Wilbrandt, in 1816, Eberhard, in 1817, and Lucae in 1819, drew attention to a like condition, which the latter two writers regarded as arising from "the *musculi-bronchiales* assisting in the production of respiration not being as yet sufficiently accustomed to the rhythmic order of the respiratory acts."† The state of the pulmonary tissue alluded to by these pathologists was soon recognised by Dugès, who, in 1821 (in a thesis), remarked upon the caution necessary to be taken in not confounding with "peripneumonic condensation a state of the lungs of some weak new-born children, in whom respiration had not been completely established." In this state "the lungs are violaceous, deprived of air at certain portions, sinking in water; in fine, dense but flabby, soft, flexible, coriaceous, slightly developed, and but imperfectly filling the corresponding side of the thorax." "M. Chaussier (continues M. Dugès), who has likewise observed the above mentioned state, regards it as the effect of congestion ulterior to birth. In spite of the weight of such authority, I cannot coincide in his opinion. I have found too much analogy between what I have witnessed in infants who have died before birth and that which I have seen in those who have died several days later." In 1823, Schallgruber,‡ in discussing the causes of death, and the post-mortem appearances found in the bodies of infants supposed to have been suffocated in bed by the mother or nurse, alluded to the unexpanded condition of the lungs, which swam with difficulty in water, and remarked that most new-born children who die, do so apparently from want of, or incompleteness of, pulmonary expansion. It is true that Schallgruber (nachtrag zur Thymus druse) regarded an enlargement of the thymus gland as the main cause of the deficient expansion, yet it is clear, from what he states, that he had formed a good notion of that condition we shall presently find to have been called *Atelectasis* In 1832 the younger Iorg, following out some views of his father, published a distinct treatise* on a particular condition of the lungs of new-born children found after death, in cases "where the first act of breathing had been imperfectly accomplished, either because they were puny and feeble, or because they had been hurried into the world before placental respiration had been altogether suspended, and the necessity for pulmonary respiration became sufficiently potent to stimulate all the muscles of inspiration'† In 1835 the subject was more fully worked out‡ by the same author, and the term *atelectasis* applied to the condition in question In the same year, also, M Rufz§ drew attention (in a note to one of his Memoirs) to an alteration (carnification) of the pulmonary tissue, which he affirmed to be distinct from hepatization, but of whose entire symptomatic value he was ignorant The lung was described as being very dense, of a violet colour, sinking in water, non-crepitant, and looking like a portion of the lung of a child that has never respired M Rufz suggested that it might be simply the result of compression of the pulmonary tissue To this writer, indeed, is generally awarded the credit of thus linking, as it were, the congenital non expansion, or atelectasis of Iorg, to the assumptory collapse of the lung afterwards (as we shall presently find) playing so important a part in the hands of MM Bailly and Legendre But such credit is misplaced, for even if we cannot go the length of admitting this to have been done by Dugès, we cannot avoid believing that our countryman, Dr Alderson, had forestalled M Rufz In a paper by Dr Alderson, upon Pertussis, published in 1830,‖ a lesion of the pulmonary parenchyma was commented on, which was said to differ from the hepatization of "peripneumony" The individual lobules were more dense, of a dull red colour, devoid of air, and sinking instantly in water Such a condition, too, was found uncomplicated with any evidence of pleuritic inflammation, the lung being dense and contracted, as if the air had been expelled, and the sides of the air cells agglutinated together Following M Rufz, MM Rilliet and Barthez, in their 'Memoir,' published in 1838,¶ described a "carnification" *not the mechanical result of effusion* This carnification resembles, at first sight, the lung of a fœtus which has never respired, or more nearly simulates the effects of inflammatory action where no engorgement is left behind From this latter circumstance it might be considered, according to these writers, as a result perhaps of chronic pneumonia. Finally, upon this point we may remark, that in 1839 Dr A Rees, in a paper published in the 'Medical Gazette,' on

---

* Die Asthmatischen Krankheiten der Kinder. Berlin, 1853.

† Mauch, op. cit., p. 164.

‡ Abhandlungen im Fache der Gerichtsarzneikunde. Gratz, 1823.

---

* Dissertatio de morbo pulmonum organico ex respiratione neonatorum imperfecta orto Lipsiæ 1832

† Hasse Pathol Anat vol i p 253

‡ Die Fœtus lunge im gebornem kind fur Pathologie Therapie und Gerichtlicke arzneiwissenschaft geschildert Grimma 1835

§ Journal des Connaissances Medico Chirurg, Paris 1835, p 101

‖ Medico Chirurgical Transactions vol xvi

¶ De la Pneumonie chez les Enfants Paris, 1838

"Deformity of the Chest in Young Children," described a condition of the lung in which the latter was "converted into a dense, firm texture, sinking in water, of a purple colour, resembling most nearly lung having suffered from compression," and which condition was considered to be the result of "chronic pneumonia." Bearing these facts—parenthetically, as it were—in mind, we now recur to the position of *lobular pneumonia* becoming established as that form of pneumonitis most frequently met with in children.

The memoir of MM. Rilliet and Barthez, before alluded to as published in 1838, received high encomiums, and was particularly adopted by MM. Sestier* and Grisolle.† In 1843 appeared the great systematic work of the former writers,‡ in which the chapter on pneumonia was (to use their own expressions) to be considered as a second edition of their earlier monograph. The high authority of MM. Rilliet and Barthez succeeded, notwithstanding the opposition on some points of MM. Berton and Barrier,§ in pretty widely establishing the following doctrines. Pneumonia in the child may become developed during perfect health, or in the course of some other malady. The former or primitive pneumonia, though frequent in the adult, is rare in children, particularly during infancy. In children the disease is generally of a secondary, whether of an acute or cachectic, character. Both lobar and lobular pneumonia occur, but the latter is by far the more common form. Lobular pneumonia is observed under three phases: first, *mammelated*, in which the diseased parts are perfectly circumscribed, and have a nucleus of hepatization, whose colour and appearance are strictly defined from those of the surrounding tissue, and readily seen to be distinct, even when the latter is in a state of engorgement; secondly, *partial*, in which the affected lobules are less distinctly separated or parted off than in the previous variety, their circumferences becoming insensibly confounded with the surrounding tissues, so that no perfect demarcation can be demonstrated; thirdly, *generalized*, in which the size of the inflamed patches becomes large, irregular, and prolonged, the centres of the lobular patches being in the second stage of pneumonitic changes, the circumferential parts in the first; these latter coalescing, it results that a considerable portion of a lobe is found pneumonitic, presenting intermixed characters of the first and second degrees of the disorder; this variety has been confounded by pathologists with lobar pneumonia. It is admitted that in some instances particular varieties of generalized lobular pneumonia very closely simulate the lobar form, so closely, indeed, we may remark, that it appears to us MM. Rilliet and Barthez have failed to afford an easy solution of the difficulty—a difficulty not less apparent to M. Barrier, who makes use of the designation *pseudo lobar*. It is not denied that both the lobar and lobular varieties may exist in the same patient, but such cases are affirmed to be rare. All the varieties of lobular pneumonia are not of a like frequency at a like age. Mammelated is much rarer above six years of age than partial, whilst generalized pneumonia is yet rarer than mammelated from six to fifteen.

---

* Leçons de Clinique Medicale du Profess. Chomel. Art. *Pneumonie.*

† Traité Pratique de la Pneumonie aux différens âges.

‡ Traité Clinique et Pratique des Maladies des Enfants, tomes i.—iii. Paris, 1843.

§ Traité Pratique des Maladies de l'Enfance. Paris, 1842.

In bringing these doctrines to perfection, the minute anatomy of the lesional changes which the inflamed lobules underwent, were of course patiently investigated, and a distinction drawn between hepatization and the *carnification* of M Rufz, which latter was nevertheless linked in a very indefinite way to inflammation, and therefore reduced in the long run to a pneumonitic lesion Considering the high authorities propagating these views, or particular modifications of them (unnecessary at present to dwell upon), and the vast opportunities offered by the continental hospitals, apparently for their substantiation, it might have been supposed that the nature of pneumonia in children was now fairly established But the truce was soon to be broken Several pathologists, more particularly in Germany, recalling to mind the observations of Billard, Cruveilhier, Duges, and Valleix, in connexion with new-born children, the case of Schenk, the views of Schallgruber, the note of Rufz, and the memoir of Iorg, intimated their doubts as to the matter being by any means settled Cruse[*] and Hasse,[†] in particular, asserted that some great mistake existed, and that neither "hypostasis" nor "inflammation" fully explained some of the peculiar lesions found after death in much of the so-called lobular pneumonia of new-born children, that even perfectly demonstrable *atelectasis* could be shown to have been confounded with pneumonia, and that the whole subject required further elucidation Hasse concluded his fourth chapter on the organs of respiration, by affirming that "the greater number of cases of pulmonary disease occurring at the earliest period of infantile life, and set down as pneumonia, may be looked upon as cases of atelectasis The last assertion is, however, to be taken with some reserve, inasmuch as in the vast lying-in and foundling hospitals (Kiwisch) pneumonia is apt to become epidemic with new-born infants, and under these circumstances to attain a numerical preponderance over atelectasis" In 1843, in our own country, Dr West[‡] maintained the frequent confusion of the symptoms and effects of atelectasis with those of pneumonia, the much more frequent occurrence—at least in Britain—of idiopathic or primary pneumonitis than the French writers admitted of on the Continent, and also the tendency of the lobular form of it to become generalized, to be greater than they allowed Dr West also maintained the frequency of the true lobar variety Nothing less than a complete unsettlement of the whole subject as related to new born children and infants seemed inevitable, in consequence of the views thus being enforced concerning atelectasis. A sort of compromise, however, was attempted by some it was admitted that a "fœtal condition" of the lungs frequently occurred in children who died a short time after birth, and in whom the acts of respiration had been very imperfectly performed and that, of course, mere atelectic symptoms and lesions were not those of inflammation But it was at the same time sought to be established, that if, under such circumstances, life had been prolonged to some extent, an inflammatory condition supervened, and then after death the effects of the two conditions were so intermixed, as to baffle all solution As to "carnification," some of its analogies to the atelectic state were admitted, one or two regarded it as a result of chronic pneumonia, whilst others denied the explanations thus offered, but could not afford any solution of the matter themselves.

---

[*] Ueber die Acute Bronchitis der Kinder und ihr Verhaltniss zu den verwandten Krankheitsformen Konigsberg 1839

[†] Pathological Anatomy Organs of Respiration

[‡] Clinical and Pathological Report on the Pneumonia of Children as it appears among the Poor in London British and Foreign Medical Review, vol xv p 543

We have thus stated sufficient to carry the subject onward to the year 1844, in which appeared the memoir of MM. Legendre and Bailly,* causing quite a revolution in the history of "lobular pneumonia." They introduce the subject by remarking upon the fact of the various writers on pneumonia, during the previous twenty years, having occupied themselves with a special alteration, called *splenisation, carnification,* &c., and that, with the exception of M. Rufz, they are quite in error about its nature; and not only this, but such is the confusion and contradiction existing, though the idea of inflammation has been common to all, that one cannot help rising from the perusal of what has been written, with the paradoxical impression that "the pneumonia of children essentially differs from that of adults, in so far that it is not an inflammation." It is then affirmed that, whatever it may be, the authors had convinced themselves "of the absence of all inflammatory action in the indurations found in the lungs of a large number of children who die from pulmonary catarrh, and often in the course of other maladies foreign to the lungs—indurations which had been regarded up to the present time as the disseminated nodules of lobular pneumonia." Having found a certain analogy between these alterations and the condition of the lungs of a child which has never respired, they were led to enquire if the pulmonary tissue of an infant could, under particular circumstances, reassume the anatomic state presented before respiration was established; this their investigations satisfactorily proved to them could be done: and upon this single but important fact are based the views they would promulgate on the question. These views they preface by an account of the lesions found in the lungs of children dying from "pretended pneumonias," and which are now asserted to be analogous to the normal condition of the pulmonary organs in the fœtus. Great stress is laid upon the fact, that insufflation can restore the affected lobules, "*à l'état physiologique.*" This character necessarily excludes in the tissue which possesses it all idea of inflammation with veritable lesion. How is it possible to admit that such could disappear, and the organ reassume its normal condition, under the influence of this single operation?" "Besides, a lung truly hepatized permits of no penetration of air by insufflation." This "fœtal state" [of post respiratory assumption] is then minutely described, and two varieties of it laid down—viz., a simple and a congestional variety. The former is met with in children of from five months to five years of age, as a complication of the larger number of diseases, particularly of bronchitis, and embraces "carnification," and "marginal pneumonia;" whilst the latter is the only form met with in new-born children, and includes the majority of instances recorded by writers as examples of simple or double lobular pneumonia. The circumstances inducing the assumption of this "fœtal state" are those which by any means hinder the free access of air into the pulmonic cells; general debility, rachitism of the thoracic walls, exhaustion from disease, inflammation of the bronchia, &c., may do this, and the consequence will be, that "a sort of retraction of the pulmonary tissue is produced, which thus being deprived by default of air of its functional activity, gradually collapses and becomes condensed" In the production of the "congestional" variety, another force is superadded—viz, "sanguineous congestion of the vascular network enveloping the cellules, a congestion having as its effect the compression of the latter" A very common affection in children is "inflammation of the bronchia and of the vesicules" to this combination the term of *catarrhal pneumonia* is appropriate, as an attendant upon or a likely contingency of the disease, is found occlusion or compression of the pulmonary vesicles, in other words, the assumption of the "fœtal state" in certain portions of the lungs The above, with its modifications, constitutes for the most part the disseminated and generalized lobular pneumonias, mucous pneumonia, capillary bronchitis, and the suffocative catarrh of different pathologists MM Bailly and Legendre, after resolving the greater mass of cases of lobular pneumonia in the way thus indicated, ask the question—if there really does exist such a disease as lobular pneumonia? This is answered in the negative, as its affirmation would involve a perversion of terms, and, moreover, the pneumonia of children is always parenchymatous, not having its *point de depart* in the mucous tissue, but in the cellulo-vascular, and in the cellular interstices separating the ultimate bronchia It would, therefore, not be reasonable to retain the term lobular as applicable to any parenchymatous pneumonia, however circumscribed the latter might be Parenchymatous lobar pneumonia occurs in children, and gives rise to two kinds of partial hepatization, "one, irregular in its form, is nothing but ordinary simple pneumonia, only of limited extent;" "the other is mammelated or truly circumscribed" Thus far is it necessary to go with MM Bailly and Legendre The opinions they have advanced have been adopted by some in their totality, by others but partially, many suspend their judgment until after further enquiry, whilst some are entirely opposed to them, and maintain much the same views previously held regarding lobular pneumonia

---

* Archives Générales de Médecine, Jan. Fev. Mars 1844.

In 1845, Dr West remarked, in his "Report,"[*] that he had repeated the experiments of the above writers, and could fully substantiate the correctness of their statements as regards the effects of insufflation In 1852,[†] he coincided with their more important views, believing that by the simple experiment of inflation more light had been thrown on the affections of the lungs in infancy and childhood than by all the writers of the previous ten years Dr West also appealed to the observations of Baly, Gardner, and Louis, illustrative of collapse and carnification of the lung in adults, &c, as tending to establish the main truth involved in the argument Though formerly coinciding in the opinion that an unusually congested state of the reticular vessels had power in compressing the air vesicles, he did not agree in this now, considering the congestion to be rather a secondary and accidental occurrence He also believed that a form of pneumonia now and then occurs which may justly be called lobular, whilst lobar pneumonia is often met with in early life, both as an idiopathic and secondary affection In 1845, Traube, in his 'Contributions to Experimental Pathology and Physiology,' described "red induration" as rather to be regarded as atelectasis than as the result of inflammation, and as produced by the accumulated mucus in the smaller bronchia. "We stand here, as it is seen, on the confines of a territory that can only be conquered for science by sufficient clinical and pathologico-anatomical investigation, combined with systematically-ordered experiments" Unfortunately for Dr Traube, as Fuchs remarks,* this territory had already been invaded a year before the latter made his pretensions to it, and all that was required was, that existing literature should have been more carefully consulted In 1846, Dr A Rees published a paper in the 'Lancet' upon "Carnification," prefaced with a reclamation against MM Rilliet, Barthez, Legendre, Bailly, and Dr West, and affirmed that to him was due the honour of first noticing "carnification" in young subjects, unconnected with effusion into the pleura Dr Rees rejected the idea of its being a result of chronic pneumonia, and stated there to be considerable difficulty in assigning its causes

---

[*] On the Progress of Practical Medicine in the Department of Midwifery and Diseases of Women and Children British and Foreign Medical Review vol xx p 525

[†] Lectures on the Diseases of Infancy and Childhood London 1852, p 168

M Legendre, after the lapse of a short time, collected his separated essays, and published them as a substantive work † In 1846, it was reviewed in the 'Archives Gén de Médecine' (t xii 4th série, p 379), the review bearing the signature of V——x [Valleix?] On the conjoint labours of MM Bailly and Legendre regarding our present subject, it was stated, that it was believed these two investigators had generalized too far, that without doubt they had indicated an important fact in showing that in certain forms of carnification of the lung no connexion with true pneumonia existed; and this they had in particular shown to hold good as regards the greater number of cases of lobular pneumonia supervening in the course of a severe malady But they had not proved that the same held good as respects the lobar pneumonia of new born children, for whoever has seen the lungs affected with this disorder will scarcely comprehend how their primitive lightness and permeability can be restored to them by insufflation, nevertheless (as it was afterwards remarked by M Valleix, in the 'Bulletin de Therapeutic,' vol xxxvi), a condensed state of the pulmonary tissue disappearing on insufflation may very often be found in very young infants Still it is an error to suppose that all the cases usually described as infantile pneumonia are of this nature In such cases, even a denser hepatization than is met with in the adult may be found, the lung rapidly sinking in water, and being quite impervious to insufflation Again, many of the cases described by MM Bailly and Legendre would not, from their symptoms during life, be by good observers considered as those of pneumonia

In 1847, the writers under the direction of M Fabre,‡ in commenting upon the views of MM Legendre and Bailly, remarked, that the anatomic details upon which they are founded are too decidedly based or dependent upon the necessity of admitting a particular structural condition of the lung, and one by no means so generally held to be correct as these pathologists seemed to assume, and M Bourgery, who has most occupied himself with this point, by no means agrees with the doctrines of Reisseisen On the other hand, Dr Fuchs§ in 1849 remarked, that it is perfectly immaterial whether the structure of the lung be such as is maintained by Reisseisen or by Rainey A most determinate opponent of MM Bailly and Legendre is found in M Bouchut * he affirms (1845—52) that the common practice of dividing the pneumonia of children into many varieties, based upon the innumerable forms of its anatomic lesions, has the great drawback of " infinitively dividing" a subject which, on the contrary, demands greater simplification He opposes the views under consideration, and maintains that no confidence is to be placed on the experiment of insufflation, for " in the child as well as in the adult, in lobular as in lobar pneumonia, in the first as also in the second and third stages, as likewise in that of grey hepatization, the insufflation of the lung is *often possible,* the air may penetrate into the pulmonary vesicles, and permit of the parenchyma floating upon water" According to M Bouchut, lobular congestion and inflammation of the lobules constitute the *point de depart* of nearly all the pneumonias of young infants, and the lobar form of complete and uniform hepatization of the pulmonary parenchyma is very rarely met with in such patients, when it does occur, also, it scarcely ever presents the same anatomic characters which are generally allotted to the *pneumonie franche* of adults It is in reality so little different from confluent lobular pneumonia, that a question of *degree* alone divides them Two forms of lobular pneumonia are laid down by the author—viz, a discrete and a confluent According to Friedleben,† true lobar pneumonia is one of the most frequent diseases of childhood, whilst the lobular form is a very rare one, very uniformly of secondary origin, and when primitive, occurs only within the first year of life We may now consider the general bearings of the subject exposed and carried forward to 1849—50, when the work of Dr Fuchs, from which we have before quoted, appeared The main points therein inculcated are as follows —True pneumonia never occurs in children below five years of age, though in them "catarrhal inflammation of the pulmonary vesicles" is frequently met with at the posterior margins of both lobes, and connected with inflammatory congestion of the parenchyma But this should be regarded rather as a consequence of death than as a contingency of life, and does not deserve the importance attached to it by M Legendre Whatever may be the nature of the "nodular," "red," and "blue-grey indurations" met with in the lungs of young children, they are not due to inflammation of the pulmonary parenchyma, they have their *genesis* in bronchitis The vesicles becoming devoid of air, a portion of the lung fails in, or is deprived of, its function, and it becomes atrophied "Legendre has rightly grasped the matter, but his appellation [fœtal state] leads to confusion, since it is held as synonymous with atelectasia " "Under the term atelectasia that condition of the lung is implied which exists before birth and before commencing respiration, and after birth and after respiration In the former case it is a normal condition, but in the latter, where air should and must penetrate the lung, it becomes an abnormal, even a diseased one the cause of the latter does not lie in the pulmonary parenchyma itself The term of 'atelectasia, as applied to it, has not been selected with due circumspection " "I designate with the term *apneumatosis* that condition which ensues when a lung previously containing air becomes void of it." "A lung in which the respiratory process has commenced and the minor circulation began, into which air has penetrated, and in which the vessels of the minor circulation have become enlarged, and contain more blood, does not retrograde into the fœtal condition, nor does it reassume the quality of the fœtal lung." But after all, according to Dr. Fuchs, M. Legendre has only described the lesion up to the beginning of its second stage, and therefore his views are wanting in completeness. The result of the investigations of Dr. Fuchs appears to be, that that which many pathologists have denominated lobular pneumonia in young children is a diseased condition of the mucous membrane of the bronchia, combined with a contingent lesion of the pulmonary vesicles, which latter exists normally in the fœtus [atelectasia, atelectasis, fœtal condition or state, &c.], or may be afterwards acquired [apneumatosis] from various causes, bronchitis being one of them, the apneumatic lesion presenting certain modifications, according to circumstances, we need not dwell upon. It was natural to expect that much interest should be evinced by those specially prosecuting the study of pædiatrics in awaiting the judgment of MM. Rilliet and Barthez upon the peculiar views promulgated by MM. Legendre and Bailly. In 1851 that judgment was given,* of which we must lay before our readers some details. The anatomic investigations of the latter pathologists receive from MM. Rilliet and Barthez that credit which they so fully deserve, but it is believed that the influence of *congestion* has been exaggerated far beyond that of parenchymatous inflammation—or, in other words, the frequency and importance of the inflammatory element have been too much limited. MM. Legendre and Bailly have also laid far too much stress on what they have termed the prolonged or chronic congestive form, and have mistaken active pulmonary congestion for it. With these provisos, however, they agree to separate entirely lobar pneumonia from lobular pneumonia, which is the catarrhal pneumonia of MM. Legendre and Bailly, and the broncho pneumonia of themselves. This they are led to do, seeing that their farther investigations in the track so ably indicated by the above writers have, without affording identical results, yet so far agreed as sufficiently to prove the necessity of the separation, though this be necessary, however, no important changes as regard the diagnosis, prognosis, ætiology, and therapeutics, will have to be made to what relates to those points of the maladies in question as laid down in their systematic treatise.† It is in the section on pathologic anatomy that the changes must be made. The labours of their colleagues, and their own verified results, lead to quite a new view of pulmonary inflammation—a view indeed which even MM. Legendre and Bailly have not completely seen through, for in their, for the most part, mere hypostatic and scarcely inflammatory congestion of the vessels, "a real inflammation of the lungs is present, which, moreover, is essentially distinct from hepatization, and is by no means only a gradation of this so common anatomic change."

---

* Die Bronchitis der Kinder Leipzig 1849 p 111

† Récherches Anatomiques et Cliniques sur quelques Maladies de l Enfance Paris 1846

‡ Bibliothèque du Médecin praticien Maladies des Enfants t i p 165 sous la direction du Docteur Fabre

§ Op cit p 68

---

* Traité Pratique des Maladies des Nouveaux Nes et des Enfants à la Mammelle Paris, 1852 Prémière édition en forme de " Manuel Paris 1845

† Archives fur Physiologische Heilkunde Heft 2.

It is an error to reduce all forms of pneumonia to a single type of anatomic alteration, regarding a difference only as existing in degree and extent. There is at least a form of pulmonic inflammation that cannot be regarded as only a grade of hepatization, and which even can no more pass into true hepatization, than erysipelas can be metamorphosed into acne Attention must be directed to two forms of pulmonic inflammation—viz, lobular congestion and partial hepatization The term *lobular congestion* is used, merely for want of a better one, to distinguish it from true hepatization it is not what is usually understood by engorgement, there is a very essential difference between them The term "fœtal condition" is not employed, because it rejects all idea of inflammation, and because it has been employed to designate two textural alterations which cannot be associated together—viz, carnification of the lung, or, rather, collapse of the pulmonary lobules, and lobular congestion The latter is very frequently met with, it may be said to be one of the most common pulmonary affections, excepting broncho-pneumonia, and where it has been present, traces of changes in the bronchia are almost sure to be found Though it may be possible that "lobular congestion" may arise without the influence of pulmonic catarrh, yet it rarely does so, and it must be maintained that lobular congestion is a consequence, or at least an accompaniment, of catarrh The lung in this affection suffers collapse of the walls of its vesicles, as in the fœtal state, and as shown by Legendre and Bailly The vesicles are devoid of any contents like plastic lymph, or any other fluidity, the diseased parenchyma, from its turgescence and swelling from congestion, requires more space, the air is thus driven out of the cells, and the walls of the latter become appressed In many cases after death, air cannot be made to penetrate all the vesicles by insufflation, this occurs, probably, when the parenchyma between them is thickened or engorged This engorgement is to be regarded "as the result of an extra-vesicular and extra vascular secretion, and must be viewed as the second degree of a true inflammation of the cellular tissue" It is difficult to conceive how this "lobular congestion" is always the result merely of hypostasis, or identical with that arising in the *agony* That the conditions favouring hypostasis favour the production of the other may be true, but it is to be maintained, that very frequently, if not always, a truly active and even inflammatory process has been in operation at the same time There may be three causes of production in operation—inflammation, hypostasis, and an hindrance to the entrance of air into the bronchia, and according to the intensity of either, so may the pulmonary congestion present somewhat different characters But this pulmonary lobular congestion, under any circumstances —partly from its specific nature, from its seat, and the condition of the bronchial mucous membrane—has but very slight tendency to give rise to plastic effusion in the pulmonic vesicles, and which characterizes lobular and primitive hepatization, nevertheless, there cannot absolutely be denied to exist a certain relationship between the two pathologic changes, and the possibility of an apparent transition of one into the other in the following way "It may happen, that under the influences of a strong constitution, of age, of the nature of a previous disease, &c, the active congestion, with its effusions, may assume a more inflammatory character, and evince a plastic tendency, so that a true extra-vesicular hepatization may follow on the lobular congestion"

---

* Einige Bermerkungen zur Geschichte der Broncho pneumonie der Kinder. Von E. Barthez, &c. und Rilliet, &c. Journal fur Kinderkrankheiten. Band xviii. Heft 5 und 6. 1851, p. 329 et seq.

† Traite Clinique et Pratique des Maladies des Enfants. tom. i.

It would be beyond our purpose to enter more fully into the details of MM Rilliet and Barthez' elaborate memoir, we shall therefore conclude our analysis of it by the following quotation, which terminates the essay —‘ Lobular congestion is, as well as partial and generalized hepatization, in genetic relation with pulmonary catarrh Upon this point the results of post-mortem investigation, in connexion with the evidences of auscultation and the progress of the symptoms, leave not the slightest doubt In rare cases, some isolated nuclei of hepatization are met with in the lung, and which cannot be ascribed to catarrh, but this is an exception which cannot enforce the complete separation of partial hepatization and catarrhal pneumonia The same reasons enforce the coalition of capillary bronchitis with congestion and hepatization The one cannot be separated from the other, the only difference lies in the less frequent occurrence of hepatization " (p 350, *op cit*)

In 1852 M Hervieux, in some remarks* upon the " effect of prolonged horizontal posture in the production of the great mortality in foundling hospitals," asserted that such posture produces sclerema, &c , from which some children die, whilst others succumb " à ces pretendues pneumonies qui ne sont autre chose que des stases sanguines ’ M Bailly has since† replied to this statement of M Hervieux, and recalled attention to the fact of his having already, ten years since, asserted that the pneumonias so fatal to young children in hospitals are due to the stasis of blood consequent upon prolonged decubitus Since then, M Bailly, having witn[illegible] disease under different circumstances, has not met with those numerous examples of bronchial catarrh generating fatal pneumonia, or of children dying from sclerema, and with their lungs gorged with blood It may be doubted, indeed, whether such affections are not entirely generated in hospitals, and also that much of what is contained in works on Pædiatrics, deduced from hospital investigation, is of any applicability to private practice Finally, we would remark, that at the commencement of the present year, Dr Gairdner, in an able article on ‘ Bronchitis, Pulmonary Collapse, and Emphysema,’‡ expressed his opinion that the physical condition of the lung in the so called " lobular pneumonia," is a form of condensation having no connexion whatever with any of the ordinary types of inflammation or hepatization, but " that it has the closest possible analogy, if not identity of character, with the unexpanded condition of the lung of the infant just born In the opinion of the writer, neither the views of Legendre nor of Rilliet afford a satisfactory explanation of the forms of pulmonary condensation under review " The *état fœtal*, or collapse of the air-cells, when occurring in a lung that has been once expanded, is in all probability a secondary lesion, and dependent, in the majority of instances, on a catarrhal condition of the bronchial tubes"—a conclusion as we have already seen, arrived at by Fuchs Dr Gairdner also maintains the opinion, " that, in some exceptional instances at least, collapse of the lung, even in its lobular form, is a disease of adult life," and that the reason of the great liability of the child to pulmonary collapse, as a consequence of bronchial obstruction, is chiefly seen in the want of resistance in the bones of the infantile chest preventing the muscles from acting on individual portions of it with the force necessary, in some cases, to overcome bronchial obstruction, and in the frequent coexisting debility and dyscrasia which are such strong predisponents to the occurrence of the lesion under review.

---

* L Union Médicale Paris 1852 Nos 139 140 † Ibid , No 149

‡ British and Foreign Medico Chirurgical Review No xxii p 452, April, 1853

We should not feel satisfied that we had sufficiently developed the history of infantile pneumonia without drawing attention to certain views very lately (1851) promulgated by M. Baron, of Paris.* It is true that they may be said more especially to relate to "œdema of the lungs" than to pneumonia; but as the author is so intimately concerned with "carnification," and which we have already found to be such an object of dispute, it is necessary to allude to the views in question. Previous to doing so, however, it may be advisable to remark, that Billard, Gardien, Berton, Rayer, Gregory, Rilliet, and others, had drawn attention to œdema of the lungs in children, as Laennec had to the affection existing in adults. "Carnification" was spoken of by some as accompanying it; Berton thought the œdema depended upon inflammation, and of the lungs more frequently than of the bronchia. Billard imagined there might exist some connexion between the cause of œdema of new-born children, and the serous infiltration of the lungs; whilst Gardien opined that this infiltration might be idiopathic, or at least as offering no evident cause for its production. By some, fluid was stated always to exude when the lung was cut or pressed, which fluid was believed to have been effused into the cavities of the pulmonary cells. In 1843, MM. Rilliet and Barthez pronounced judgment, based upon the observations of 77 cases. It was affirmed that the causes of the pulmonary œdema were those common to all dropsies, and only special as regards the lungs in certain very rare cases. That in all probability there existed a vesicular and an interlobular œdema, but that this is a very difficult point to settle, and one in reference to which no special investigations had been instituted. "On section of the lung an abundance of serous fluid escapes"—"placed in water, the pulmonary tissue always swims, either wholly or in part"—"the fluid is mixed with air—when the œdema coincides with pneumonia, or with carnification, but then these two affections merit the appellation of œdematous." In 1846, MM. Legendre and Bailly discussed the matter relative to the affection occurring after scarlatina, and stated it to be a dropsy of the interlobular cellular tissue, and not to be intra-vesicular œdema. By this interlobular cellular infiltration the air cells become compressed, giving rise to the dyspnœa experienced by the patient. Puncture of the lung allowed the fluid to escape, and insufflation was practicable, and caused the pulmonary parenchyma to reassume its natural tension, crepitation, and colour. In 1847, Dr. West stated his opinion that he had no doubt M. Legendre was correct in his opinions. In 1851 appeared the elaborate memoir of M. Baron "On Carnification of the Lungs in Children, and on its distinction from Hepatization." The author commences by remarking upon the fact of the former lesion having been by some considered as equivalent to the latter; by others, (Valleix and Rufz,) as different to ordinary hepatization, but as yet undetermined in its nature; and by Legendre and Bailly, and their school, as the consequence of the non-penetration of air in a given portion of the lung, which thus retrogrades, to a certain extent to the fœtal condition.

---

* Gazette Médicale de Paris 1851 Nos 46 47, 48 52 also Journal fur Kinderkrankheiten Band xviii Heft 3 und 4, from which we quote.

This latter theory, although capable of application to a large number of cases, is as little applicable to the explanation of all as the theory of inflammation Hitherto it has been the case, that the subject of carnification has been developed only from its occurrence in the pulmonary organs. But it occurs elsewhere, and it can only receive its proper development from its general consideration, the more particularly as its character and causes are everywhere identical. M Baron then gives the details of 31 cases, and comments upon them The youngest patient was five days, the eldest 73 years old, the majority of the patients were children Carnification is held to be most common in new born children; after the fifth year it is comparatively unfrequent in occurrence, whilst in adults it is very rarely met with, but in aged people again becomes more common, though still less so than in early life The characters of this carnification of M Baron differ, however, from those given by M Legendre as illustrative of his "fœtal state,' both simple and congestional From "engorgement" it also differs, as well as from hepatization, nor does it include the discrete and confluent pneumonias of various writers In nearly all the cases it was found in connexion with the remains of serous effusion, either in the carnified organ itself, or in other parts of the body The lesion has its genesis in the effects of such effusion The fluid infiltrated into the organized tissue ceases by degrees to be isolated or distinct from such tissue, but gradually becomes identified or in intimate connexion with it By this the tissue loses its normal character, becomes hardened and carnified, whilst the infiltrated fluid loses by degrees its normality When the carnification has attained its maximum development no more fluid can be pressed from the part The character last attained to is that of *induration* In a very small number of cases whilst there was carnification of one lung there was evident inflammation of the other Were such cases more frequently met with, considerable ground might exist for believing in the inflammatory nature of the lesion in question, or at any rate for thinking that inflammation had some sort of causative connexion with the latter But at present we must assume such connexion to be but of slight intensity *Splenisation* depends (as does carnification) upon the infiltration of fluid, and its development is favoured by conditions differing very slightly from those favouring the latter Between splenisation and commencing carnification there is such similarity, that it may be often difficult to say which lesion we have before us Still there are distinctions In pulmonary carnification the walls of the air cells, though appearing to be so closely appressed as to destroy vacuity, are not so in reality, as insufflation can prove The contrary is the case in hepatization,—the cells cannot be inflated, in the latter, the changes are internal, so to speak, whilst in the former they are external, in regard to the air-cells

ART V

On a Case in which the Sarcina Ventriculi, seu Sarcina Goodsirii, was found in the fluid removed from the ventricles of the human brain By WILLIAM JENNER, M D, F R C P, Professor of Pathological Anatomy, University College, Physician to the Hospital for Sick Children, &c

THE sarcina Goodsirii has been found, not only in adults and children, but also in the lower animals—viz, the rabbit, the dog, and the tortoise* It is by no means rarely present in the matters ejected from the human stomach and intestinal canal In 1847, Heller† described a case in which he discovered sarcinæ in the urine, subsequently, Dr Mackay‡ detected them in the same fluid, and Virchow§ and Zencker‖ in the lungs

To all of the last-mentioned cases it has been objected that the sarcinæ were in reality derived from the stomach, that when found in the urine, some of the vomited matters had been accidentally mixed with that fluid after its escape from the bladder, that when found in the lungs, the sarcinæ had passed into them during the act of vomiting The more recent observations of Heller¶ have, however, placed beyond a doubt the fact that sarcinæ Goodsirii are occasionally present in the urine when that fluid is free from admixture with vomited matters In one of the two cases last described by Heller, the urine was examined for many weeks in succession, and a sediment in it of an inch in depth sometimes observed, composed of sarcinæ only, or of sarcinæ mingled with a little carbonate of lime The following case lends support to the foregoing evidence in favour of the opinion that the stomach and intestinal canal are not the exclusive seats of the development of the sarcinæ Goodsirii

A boy aged four years was admitted under my care into the Hospital for Sick Children, June 21th, 1852, he died July 3rd The child was of fair complexion, well made, and moderately stout His illness was reported to have commenced on June 17th, with pain in the head On the 19th, he was taken as an out-patient to King's College Hospital, subsequently to that time he never complained of headache, only he seemed heavy He did not keep to his bed till the 23rd After his admission into the children's hospital, the most prominent symptoms were, drowsiness, talking in sleep, an irregularly diffused scarlet rash on the skin, redness and swelling of the tonsils, with a white patch on the left tonsil, frequent pulse, sordes about the teeth, and some dryness and browness of the tongue On the 30th my notes say, 'Almost incessant grinding of the teeth, seems quite sensible when awake, asks for the cold wash to be applied to his head"

Between the 30th of June and the day of the child's death,—i e, July 3rd,—restlessness, strabismus, inequality of the pupils, and redness of the conjunctivæ. General convulsion commenced about four A.M. on the 3rd, and continued till the child's death at half past four A.M.

---

* Canstatt's Jahresbericht Dritter Band 1850

† Eine eigenthumliches Harn sediment Archiv fur physiologische und pathologische Chemie und Mikroskopie Heft 4

‡ Lectures on Clinical Medicine Dr J H Bennett July 1851

§ Schlossberger Die Sarcina Wurttemberg Corresp Bl N 26

‖ Henle's Zeitschrift Band iii

¶ Archiv fur physiologische und pathologische Chemie &c 1852 p 30

The examination of the body was commenced ten and a half hours after death. The vessels of the dura mater were more filled with blood than is usual. Numerous semi-transparent grey granulations were seated *on the arachnoid lining the dura mater*, and a few similar granulations on the visceral arachnoid. The arachnoid itself was dry; the cerebral convolutions were flattened; the minute vessels of the pia mater, on the surface of the convolutions, were abnormally injected with blood. In the grey matter of the cerebrum were about fifteen masses of yellow tubercle—the largest was oval, half an inch by a quarter of an inch—the smallest about the size of a very large pin's head. The pia mater, dipping between the convolutions, was studded with grey granulations. Four ounces of colourless serosity were removed from the lateral ventricles—that which first escaped on opening the ventricles was transparent, that which flowed towards the last was turbid. The fornix and septum lucidum were white and of a creamy consistence. Viewed from within, the floor of the third ventricle was highly vascular. The membranes covering the base of the brain were opaque, tough, and loaded with serosity. The fluid removed from the ventricles was alkaline and albuminous; after standing twenty-four hours the deposit of albumen constituted one seventh of the fluid tested. In the cerebellum were several masses of yellow tubercle.

Microscopical Characters of the softened Fornix and Septum Lucidum.—There were no granular corpuscles, no free fat granules, detected in the softened septum lucidum and fornix. In the cerebral substance, adjacent to some of the tubercles, were numerous large granular corpuscles.

The peritoneum was studded with grey granulations; it was abnormally vascular. Grey granulations studded the pleura, pericardium, and lungs; the bronchial glands were stuffed with tubercle; the liver was dotted throughout with small transparent, grey granulations; yellow tubercles in considerable number were found in the spleen and kidneys; there was an ulcer on either tonsil. The small and large intestines were extensively ulcerated; the edge and floor of some of the ulcers were covered with tubercles.

July 5th, 11 A.M.—The fluid removed from the lateral ventricles of the brain was examined more particularly than it had previously been. After its removal from the cerebrum, on the 3rd, the fluid had been kept in an open glass vessel, in a large, light, and airy room. It turned turmeric paper brown; its odour was sickly, brain like, not ammoniacal; it seemed as if just beginning to decompose. There was a little sediment at the bottom of the vessel.

A drop of the fluid containing some of the sediment was examined with a magnifying power of about 200 diameters; the following objects, *and those* ONLY, were contained in it.

1. A considerable number of spherical bodies $\frac{1}{4000}$th of an inch in diameter, of a pale yellowish colour, (blood discs altered in form?)
2. Square bodies, each side of which measured $\frac{1}{2000}$th of an inch; some were a little larger than this, others a little smaller. The surface of each body was divided by cross lines into four equally sized compartments, and each of these quarters was again divided into four

In some of the bodies the lines producing the secondary quarterings were well, in others imperfectly, marked, while in others these secondary quarters were themselves divided by cross lines into four parts The angles of the bodies were somewhat rounded Some of these bodies were, my notes state, "as well formed sarcinæ as I have ever seen"

3 Oval bodies about $\frac{1}{5000}$th of an inch in breadth, and $\frac{1}{2500}$th of an inch in length, distinctly divided by a transverse line into two equal parts, each of these two parts being very obscurely divided by a longitudinal line into two other parts

The fluid removed from the pericardium had been standing side by side with that removed from the ventricles of the brain, it was alkaline, but contained no sarcinæ

It was a question when this child came under observation, whether or not he was suffering from scarlatina The fatal termination was evidently the result of acute tuberculosis Unfortunately, the fluid from the ventricles of the brain was not examined for forty eight hours after its removal from the body The questions, therefore, arise—Were the sarcinæ in this case developed after the fluid in which they were found was taken from the cerebral ventricles? Were they developed after death, but within the body Did they exist in the ventricles while the child was yet living? To whichever of these questions the answer be in the affirmative, the occurrence of sarcinæ under the conditions mentioned is remarkable

The fluid in which sarcinæ have been found in the stomach has been on all occasions acid, and in the same situation vinous fermentation has been their constant concomitant So invariably have the torulæ cerevisiæ, and sarcinæ Goodsirii been found together, that Simon of Halle[*] has maintained that the latter are merely an advanced stage of the development of the yeast plant In the case I have detailed, as in one of Heller's cases, the fluid in which the sarcinæ formed was alkaline, and in it there was no trace of torulæ, and no evolution of gas Supposing the sarcinæ to have been developed in the fluid after its removal from the body, then this case stands alone, inasmuch as they have never heretofore been known to form, except in the interior of the bodies of animals

As to the stages in the development of the sarcinæ, they would appear from his case to be briefly these A simple cell is divided into two parts by a transverse line each of the two cells thus formed are again divided into two by a longitudinal line, each quarter of the primary cell subsequently experiences the same changes as the primary cell itself This description of the mode of development of the sarcinæ differs *in toto* from that given by Simon of Halle,[†] who states that the primary cell increases its size and changes its form by the formation of nuclei in its interior, and, as widely from that given by Pockel,[‡] who affirms that the increase in size of the primary cell is partly the effect of endogenous cell formation, and partly of gemmation, while comparatively, it differs but little from that given by Frerichs[§]

---

[*] Ueber die Entwicklung der Sarcinæ aus dem Hefenpilze Virchow's Archiv Band ii

[†] Loc cit

[‡] Nonnulla de Sarcina Goodsirii, Wirceburgi

[§] Ueber Sarcina Ventriculi Haser's Archiv, Band x.

PART FOURTH

Chronicle of Medical Science*

ANNALS OF MICROLOGY

By Robert D Lyons, M B, T C D, M R I A
Honorary Professor of Anatomy to the Royal Dublin Society &c &c

---

Part II — Pathological Micrology

HISTOLYSIS, OR DECAY AND DISSOLUTION OF TISSUES

INTERMEDIATE between the normal conditions of the tissues in health, and the changes which they undergo in disease, a class of structural alterations may be placed, to which as yet no distinct position has been assigned, in either anatomical or pathological studies We allude to the *alterations in form* which occur in the process of decay, as we trace an organized body through the several stages of putrefaction to its final dissolution

Some investigations on this interesting and important class of subjects have been undertaken by the writer †

Several instances having come under his notice in which structures submitted to microscopic examination appeared to have undergone considerable alteration by reason of a more or less advanced condition of decomposition, he was led to institute a series of observations for the purpose of discovering the order (if any) of the morphic changes which take place in the passage of organized bodies through the several stages of putrefaction to their final dissolution and decay, until they return, "ashes to ashes," and "dust to dust" As he believes that the researches he has already made warrant him in stating that a certain order of morphic changes is brought about, he has introduced the term HISTOLYSIS, to designate the morphic changes of putrefying tissues, the use of a single word being convenient for the purposes of description and reference

Not only will the study of histolysis be found interesting in itself, as a portion of scientific inquiry, presenting as it does several beautiful and unexpected phenomena, but, moreover the knowledge thereby acquired admits of several valuable and practical applications Thus, it affords most favourable opportunities for the study of the intimate structure of complicated normal textures, which are thus, as it were unfolded to our view by a process of natural dissection, in which the least possible violence is done to the most delicate parts In the memoir referred to the writer details some of the observations which he has already made, they relate to cutaneous structures and fat, some varieties of human and animal blood, and muscular fibre

---

* In the next number of this Journal Quarterly Reports on Medicine Surgery Midwifery and Medical Jurisprudence will be commenced in place of the abstracts at present given The Annals of Micrology and Physiology will be continued half yearly Every third or fourth number a Report on Materia Medica and on General Therapeutics will be given and as occasion may require every year or every eighteen months a Chemical Retrospect will be published The Therapeutical Record will be continued as at present and a section will be introduced for miscellaneous medical topics for which a place may not easily be found under the above mentioned headings

† Researches on the Primary Stages of Histogenesis and Histolysis by Robert D Lyons M R I A &c (Proceedings of the Royal Irish Academy, vol v part 3 1853)

1 *Integument* —This specimen had undergone change for four months The epithelium of the cuticle was entirely destroyed its place being supplied by a soft, pulpy mass, which presented an amorpho granular structure under the microscope

2 *Subcutaneous Structures after 6½ Months* —They had the appearances of what is known as cadaveric fat, under the microscope, D 900, there was seen an abundance of minute but well defined granules, with fine, dark, well marked border, and clear, transparent centre, they were arranged in masses, isolated, and in groups, small granular corpuscles of different sizes and a few oil globules, very large oval, pale, semi opaque cells or vesicles, they had no nuclei, their borders were clear and well defined, but most of them presented fissures, some as many as seven or eight, which extended inwards for about a sixth of the smaller axis, a very remarkable linear or moniliform arrangement of granules presented itself in certain parts of the field, sets or rows of granules being arranged longitudinally, parallel, or convergent, and apparently in connexion with very faint subjacent striations in the same direction The writer regards this as an instance of granular disintegration of a band of fibre,- lastly, numerous stellate crystals, apparently of the fatty acids, lay scattered over the field, some also being included in cells, and appearing to be the stage of *lysis* of their previously granular contents, preparatory to the final rupture and dissolution of the including membrane

Blood —He has met with specimens of blood in which changes took place with great rapidity, in one specimen after twenty six hours, he found the hæmatine had escaped from numerous corpuscles, and had assumed the shape of granular masses, heaps and crystals (hæmatoid crystals of Virchow)

4 *Histolysis in Fluid Blood* —Blood of duck after two years (kept in a bottle) There was presented an assemblage of forms widely different from those of the natural blood, none of the characteristic elliptical corpuscles of this animal's blood was to be found There existed abundance of granules, granular corpuscles and spherical vesicles of moderate size There were also numerous prisms and needles of hæmatine, and large irregular hæmatine masses He has since examined a specimen of human blood five years old, it was fluid, and the appearance even similar to those now given

5 *Blood of Salmon* —Of same date as last, and presenting nearly the same appearances the granular bodies were, however, much larger, there existed also very large cells, including vesicles, and masses of granular base Cells, including stellate crystals, were also visible He is disposed to think that these forms resulted from the greater quantity of oil in the blood of fish, which, with the albuminous element gave abundant means for these formations

6 *Histolytic Changes in Muscle* —The specimens examined had undergone the process of putrefaction for a considerable time, yet, by the aid of the microscope, the nature of the structures admitted of demonstration in a clear and positive manner though, by the unaided sight no amount of careful study would have sufficed for their recognition and identification Under a power of 900 D, the following appearances were presented —A semi fluid granular mass, tinged of a light brown colour, in which granules, granular corpuscles, and spherules, were visible, here and there lay larger and smaller masses of elementary fibres, many of which retained, either in whole or in part, their characteristic striæ, but presented internally more or less change There were also various stellate and acicular crystals, some within cells, others free on the field

These results of the histolysis of muscle are incomplete, yet the author thinks they give indications of the mode in which the tissue breaks up *

---

* At p 455 of the present number will be found a quotation embodying the general results of the inquiry

In connexion with this subject, we may notice one or two observations of other authors, made evidently without any definite object in view, and with which the writer was quite unacquainted at the date of publication of his researches. Virchow[*] gives some account of an examination made by him of an amputated leg and foot which had lain for a considerable time in maceration; the parts were converted into a soft white substance, having but little smell, which had all the characters of adipocere, even its fusibility. Gibbes (quoted by Virchow) appears to have made some observations on macerating bodies.[†] Quain, also, has engaged in a similar inquiry.

DEGENERATIONS.

Fatty Degenerations.—This important class of subjects has been very fully considered elsewhere,[‡] and we shall now merely refer to our former pages. There appears good reason to believe that the process of fatty degeneration is the mode by which the uterus *post partum* is restored to its normal dimensions. Professor Retzius has already investigated this subject, and more recently, a memoir has been devoted to it by Dr. Heschl.[§] This author states that the proper substance of the uterus undergoes so complete a transformation into molecular fat, that not one single fibre of the organ existing previous to child-birth remains behind. This transformation he has not observed to commence before the fourth or sixth days, and not later than the eighth. In the single muscular fibres the process begins in many points at once; the outlines become pale, and there appear yellow granules, which, when the ends of the fibre-cells are thin, lead to their early dissolution.

ABNORMAL CONDITIONS OF BLOOD.

Leukæmia Lienalis (*Leucocythemia*, Bennett).—Dr. J. P. Uhle[||] has published the results of his investigation of a remarkable case of this affection of the blood. The patient had from early life been the subject of various attacks of disease. Signs of enlargement of the spleen were observed for the first time four months before death; the tumour increased in size until finally it reached the mesial line, and extended from the ninth rib to the crest of the ilium; the liver was but slightly increased in dimensions. Blood withdrawn by cupping presented a dark brown-red colour, which, however, became clearer on exposure to the air, and formed a clear reddish-brown coagulum. Under the microscope, the colourless corpuscles were found to be remarkably increased in quantity, and on being counted, appeared to equal in number the coloured corpuscles on the field. The latter presented nothing abnormal; the colourless corpuscles however, differed much in size, some being enormously large, equal to double their usual dimensions; all of them presented regular round borders, and generally a pale granular surface. On the addition of acetic acid, two to three, seldom four, large nuclei were brought into view, presenting the greatest variety in their manner of arrangement. When the blood was treated with water, the colourless cells became much swollen, the nucleus, however, undergoing no change.

The blood was again examined two months subsequently, and ten days before death. After rest for twenty-five hours it presented a perfectly clear serum, occupying one-fourth of the entire mass; beneath lay a small soft coagulum, covered with a clear red thin layer, transparent at its borders, under which there was a somewhat thicker dark red coagulum. Under the microscope the quantity of the colourless cells appeared to be about $\frac{1}{3}$ to $\frac{1}{5}$ of the coloured. The size of the colourless cells was more uniform than on the former examination, their diameter varying between 0.009 and 0.013‴. After death, the blood showed many differences in regard to the colourless cells, in portions taken from different vessels Blood taken from the left side of the heart (which, however, appears to have been mixed with some from the jugular vein) presented appearances very similar to those found in that last drawn during life The coloured and colourless cells were in the proportion of 5 3 to 3 2, the colourless showed no remarkable differences in size, their average diameter being 0 011^{mm} In the blood of the splenic vein, the colourless corpuscles appeared to be as numerous as the coloured, if not more so, most of them were finely granular, in many the cell membrane was difficult to be seen, and without the addition of any reagent a nucleus, usually single only rarely bi or tri partite, could be distinguished A few pale spindle shaped bodies, with oblong nuclei, were occasionally met with, also a molecular mass, apparently a coagulation product, with fine punctiform granulations, which surrounded these bodies No blood corpuscle holding cells were observed In the blood of the vena portæ, the colourless cells were from ⅓ to ½ the quantity of the coloured In both the last named vessels, their average diameter was 0 009 to 0 011^{mm} In the clear reddish grey upper layer of the coagulum from the basilar artery, there were to be seen only colourless cells embedded in fibrine In the blood from the jugular vein, the colourless cells were of extraordinary size, measuring 0 013 to 0 015^{mm}, they were in general darkly granular, showing a nucleus only on the addition of water or acetic acid The thyroid gland, as well as the lymphatic glands generally, presented only their usual elements The author regards this case as a very good example of pure *Leukæmia Lienalis* as there was no process of purulent formation either in the external or internal organs, no loss of blood, no alteration in the lymphatic glands, and no intermittent fever To determine definitely the relation of this blood change to hypertrophy of the spleen he suggests the necessity of a careful investigation of the blood in cases of slight hypertrophy of the organ, and also the estimation of the size of the spleen in those cases in which a slight increase in the quantity of the colourless corpuscles has been already observed (pregnancy, puerperal states, &c)

---

[*] Verhandlungen der Phys. Med. Gesellschaft in Wurzburg. Band iii. Heft 3. 1852.

[†] Philosophical Transactions. 1 94.

[‡] British and Foreign Medico-Chirurgical Review. April and July, 1853.

[§] Researches on the Condition of the Uterus after Delivery, by Dr. R. Heschl, Vienna: translated from the German. Dublin, 1853.

[||] Virchow's Archiv, Band v. Heft 3, p. 276.

Lymphatic Glandular Hypertrophy, without Leukæmia —In a note to the foregoing communication, Virchow states that he has met with a case of most enormous general lymphatic glandular hypertrophy, embracing the axillary, jugular, and other glands, in which no observable alteration of the blood existed

Leukæmia and Pyæmia —Dr Griesinger* has investigated the relative proportion of the colourless corpuscles of the blood in cases of leukæmia, in different parts of the vascular system†

An important case of leukæmia has been very ably reported by Leudet,‡ who gives also a succinct general resume of the researches hitherto prosecuted by other observers Holland§ has observed leucocythemia in two cases in which the thyroid gland was enlarged, while the spleen and lymphatic glands generally preserved their normal condition In the second case, the increase in number of the white corpuscles was well marked in the venous blood (that of the pulmonary artery), though it was not observable in the arterial (that of the aorta)

In connexion with this subject we may consider the researches of Moleschott‖ on the results with regard to the variations in quantity of the corpuscles of the blood, produced by removal of the liver and spleen His experiments were performed on frogs, and they have led him to some interesting deductions Having arranged a suitable apparatus, he was able to estimate the quantity of carbonic acid produced by frogs before and after excision of the liver, the mean of his observations gives only half the standard quantity of carbonic acid for frogs after removal of the liver With reference to the corpuscular elements of the blood, in conjunction with Donders, he had on a former occasion determined the relations of the colourless to the red corpuscles in the heart s blood of the frog as 1 to 8 The mean of nineteen observations of the blood after removal of the liver, gave the numbers of the colourless to the red corpuscles as 1 to 2,24 In some cases life was maintained for many days after the excision of the liver, till the thirteenth day in one instance Removal of the spleen seemed to produce but little inconvenience to the animal this author says that after this operation he scarcely lost a single frog The results with regard to the numerical proportions of the corpuscles are curious The proportion of the colourless to the coloured after removal of the spleen appears to have been lessened The mean of twenty two observations gives for the number of the white to the red corpuscles in the blood of the heart 1 to 9,06 When both liver and spleen were removed, then the influence of the removal of the liver predominated thus, on the fifth day after the operation, the colourless corpuscles of the heart s blood were to the coloured as 1 to 1,35, on the sixth day as 1 to 2,69, mean 1 to 2,02

---

* Virchow's Archiv Band v Heft 3 p 391 † See Chronicle, No 22 July p 269

‡ Gazette Médicale de Paris, No 24, 1853

§ Quarterly Journal of Microscopic Science No 2 1853

‖ Muller's Archives, Heft 1, 1853 See also our last number p 252

The author believes that by the removal of the liver and consequent increase in the relative proportions of the white corpuscles of the blood, we are enabled to trace the development of these bodies into the coloured, with much greater facility than under ordinary conditions Some figures accompany the paper, illustrative of his views He believes to have followed the changes in form of the colourless cells, with division and disappearance of their nuclei, to the formation of the elliptical vesicle which he considers it erroneous to regard as nucleated The body described as a nucleus he conceives to result only from coagulation of the contents of the vesicle, and to be entirely absent in fresh and perfect blood-cells in the frog

M Chaumont* has conducted some experiments on the action of chloroform on the blood corpuscles He finds that a gradual solution of the cell wall of the red corpuscles is effected and suggests the following explanation of the reaction The effect of chloroform is like that of acetic acid but is produced more slowly Ether has no similar effect Regarding the composition of these substances, he conceives that the solvent effect is manifested in the direct ratio of the electro negative nature of the reagent Thus acetic acid dissolves the cell walls speedily, chloroform dissolves them but more slowly, ether does not dissolve them at all The action is thus directly as the electro negative natures of the reagents for albuminous, and the opposite for the case of fatty substances contrary to the opinion of some observers, he finds that the colour of blood is much heightened by chloroform Venous blood assuming a brilliant arterial hue, in some cases it even returns the bright colour to blood stains on cloth, &c

MISCELLANEOUS LESIONS

Pathology of Bronchio pulmonary Mucous Membrane —Dr Black† has continued his researches on this subject He recognises three forms of acute inflammation of the bronchio pulmonary membrane 1 Simple acute epithelial bronchitis 2 Bronchitis involving the submucous tissue 3 Cellulitis, or inflammation of the epithelium of the pulmonary cells In the first form, the secretion examined microscopically, consists of well formed mucus corpuscles (young unflattened epithelial cells?), mingled with epithelial or basement patches, floating in a viscid fluid menstruum The epithelial patches are produced by a blighting of their cells, caused by the suspension of the natural fluid transudations from the blood Sometimes the patches are formed entirely of epithelial cells having attained an almost perfect development, but which yet cohere by their edges Occasionally, the action of the cilia can be seen on the surface of mucus corpuscles (?) which have just been expelled

---

* Proceedings of Edinburgh Physiological Society Edinburgh Monthly Journal April 1853

† Edinburgh Monthly Journal of Medicine March April May, and June, 1853

When in the second form, exudation takes place into the submucous tissue, it coagulates by virtue of its contained fibrin, and a nutritive matrix is thus formed similar to the basement membrane in the healthy condition and action of that structure, germinal centres are produced by molecular aggregation of the fibrin, which subsequently assume the shape and endowments of cells Exudation masses, cells, and molecules of fibrin, epithelial patches and casts of the tubes, are present *invariably* (lec^{m} inct^{m}) in the exudation during its early stage, they are therefore diagnostic of the existence of inflammatory exudation The conversion into pus cells, the author supposes to be due to the action of oxygen in the structures of the plastic corpuscles, by which they undergo an *adipocerous degeneration* Hence the origin of the fat which invariably forms a chemical constituent of pus and hence the colour of this latter fluid as a consequence of the saponification of such fat by the alkaline present in the exudation

In *pulmonary cellulitis*, a term used to designate inflammation confined to the epithelium of the pulmonary cells (air cells), when the pathological condition is fully established, the sputa present the desquamated epithelial cells, well developed mucus corpuscles, and a few points and masses of coagulated exudation Variations will be observed in advanced forms of the disease, the cellular elements being more or less abortive When a constitutional peculiarity in particular diathesis, or another disease is associated with cellulitis the exudation from the pulmonary membrane frequently contains evidence of the presence of certain products dependent on the diseased condition of the system Thus in the uric acid diathesis the author has observed urate of ammonia and uric acid in the sputa of cellulitis, in the oxalic acid diathesis he has seen distinct crystals of oxalate of lime, and occasionally cystine, while in jaundice, cholesterine and the colouring matter of the bile (biliphæin) were found He believes that such morbid products are eliminated by the epithelial cells of the membrane, that they therefore appear as accidental constituents of the mucus cells, that they likewise escape in the exudation from the denuded surfaces of the basement membrane, and that they are never present in the exudation cells That the bronchio pulmonary membrane can thus act the part of an excretory organ, but that this peculiarity of action is dependent rather on the particular character of the blood than on any selective power inherent in the epithelial cells That before such eliminative action takes place an undue accumulation of morbid products occurs in the blood, that the action of the different organs intended for the elimination of such morbid products is for the time deficient, and that on its becoming more vigorous, the sputa regain their simply morbid character

Inner Callus, its composition and significance—Dr Ulrich Hilty* has investigated the formation of callus within the medullary canal, in fractured bones His experiments were conducted by inserting pegs of ivory and silver wire into the tibia in cats and rabbits In all the cases he found a deposit of inner callus, distinguishable from the surrounding bone by its white colour On microscopic examination it showed numerous irregular vascular canals, in continuity with those of the old bone, in the bony mass itself there was to be seen a turbid hyaline substance with round and elongated bone corpuscles, having long and tortuous radiating canaliculi

Inflammation of Joints—Dr F Fuhrer† has prosecuted some investigations into the different forms of inflammations of the joints Some interesting results are given with reference to the changes produced in the structures of the diseased joints He believes that the softening and removal of the cartilage is brought about principally by a proliferous growth of cartilage cells In hypertrophy of the cartilage there takes place, under certain conditions, a superficial fibrillation of the basic structure, and some lamellæ of the fibrinous cartilage may assume a membranous appearance The bony eminences thrown out round joints, in *Arthritis chronica sicca*, are not entirely owing to pressure and excavation of the joint cavity, or to expansion of the head of the bone but are mostly new deposits, consisting of bone and cartilage Fatty degeneration of the muscles and nerves in the neighbourhood of the diseased joints was observed, also a fatty degeneration of the bone corpuscles In two cases there was found a fibrous thickening of the nerve sheaths, and a fibrous degeneration of the muscular fibre in one instance

---

* Henle und Pfeuffer Zeitschrift Band iii Heft 2

† Virchow s Archiv Band v Heft 2, p 151

Dermoid Cysts Plastic Heterotopia —Lebert* has investigated this subject in a series of valuable memoirs laid before the Societé de Biologie M Lebert acknowledges his obligations to the essay of Meckel (1815), on the abnormal development of hairs and teeth, to the work of Voigtel and to the essay of Stahlberg (1842) According to the researches of these authors, confirmed by his own, the right ovary seems to have a peculiar predisposition to the formation of these cysts He concludes, from the study of the structure of these dermoid cysts, that they are the result, in all cases, of a tegumentary heterotopia, and not of any modification of the products of conception Proceeding from without inwards we perceive first a more or less vascular fibro cellular envelope, on the internal surface of which there exists either a fibrous investment or an osseous layer, which is nothing but a calcification of the fibrous laminæ If, however, the internal investment have not undergone a fatty or calcareous degeneration, there may be observed an epidermis, most distinctly characteristic, and having the greatest analogy with the surface of the skin, it is formed by irregular polygonal scales, of a fortieth or fiftieth of a millimetre in diameter, and furnished with a small nucleus In some instances the cells, though filled with fat, are recognisable by their pavement arrangement Under this layer Lebert has recognised a well organized derma, with an areolar disposition of its fibres, and in some parts furnished with papillæ Sebaceous glands also exist he has been able to recognise their lobules and excretory ducts in some instances Multitudes of small pores and infundibula also occur Kohlrausch has even met with sudoriparous glands Hairs are found either free or implanted of the former some still retain their bulbs, frequently these hairs appear implanted when they are really only adherent, sometimes their implantation is complete through the entire wall The included fatty mass has been long recognised as a sebaceous fat The fall of the hairs allows them to become mixed up with the fat, and thus are formed the balls and masses of hair and fat so often met with in these cysts Teeth are also met with, in many cases in large numbers in one instance more than 300 were found Molars, canines, and incisives, may be met with, either separately or variously combined, there does not appear to be any reason for admitting the statement of Meckel, that the normal arrangement is followed The roots offer no greater variety than is found to prevail at the different phases of evolution in normal teeth, and the same may be said with regard to the dental cavities, which, in point of structure, were found to present the same histological elements, in the enamel, ivory, and cementum, as the natural teeth The pathological history of these cysts, the various morbid alterations they undergo, the openings which they effect into the cavities and tubes in whose neighbourhood they are found, are all considered at length in the concluding portions of these elaborate memoirs Rejecting the doctrines of fœtal inclusion and ovarian pregnancy M Lebert concludes that the origin of these tumours is due to a force which he calls *plastic heterotopia by which simple or compound tissues, and even more complex organs, may be formed in all parts of the body when in the normal state no such structures can be recognised*

Lyons has met with a case of dermoid cyst in the testis (right), the quantity of fat and waxy matter and hair was very considerable On being opened the cyst gave exit to a thick fluid of the consistence of butter milk, but of a creamy, yellow

---

Gazette Médicale Nos 46 51, 52 1852, Nos 9 12 13 1853 colour When cold it became of the consistence of marrow, which, in general appearance, it very much resembled The testis was unaffected, and lay behind the tumour

TUMOURS

Gelatinous Cystoid Growth —Professor Virchow* has given the conclusion of the pathological history of a compound gelatinous cystoid growth, which exhibited most extraordinary powers of reproduction, having been extirpated by Professor Textor not less than seven times, after which it appeared for the last time and proved fatal The subject of recurrent tumours, and the important surgical questions connected therewith, are of such high moment, that we do not feel it necessary to apologize for the length of the extracts we purpose making from Professor Virchow's paper

In this extraordinary case the skill and courage of the surgeon were only equalled by the noble fortitude of the patient In the first instance, the disease presented itself as a tumour on the shoulder blade, which was removed by sawing off a considerable portion of this bone below the spine The growth soon re appeared, and was again extirpated, subsequent to which it grew again, and a third operation was performed It is unnecessary to pursue the history of the case through each period of re appearance of the growth and operation for its removal The seventh and last operation was performed in February, 1851, every precaution being used to ensure the complete removal of the tumour, and all suspicious structures in its neighbourhood In the winter of 1851 52 the patient came again to the hospital, but the growth had now reached such dimensions that for its complete extirpation, it would have been necessary to remove the shoulder joint and scapula A puncture was made in one portion of the growth, which gave exit to a large quantity of a reddish brown and yellowish gelatinous fluid On the shoulder blade there were two dense elastic tumours, each of the size of a fist The region of the left shoulder was remarkably swollen and discoloured, and a dense globular body, of the size of a (Borsdorf) apple, could be felt between the acromion and the clavicle The left arm was swollen and œdematous to some distance below the elbow and in some places gave signs of fluctuation The tumours increased considerably in size, discharge continued from the wound, the skin on the back became gangrenous, insatiable thirst, sweating, and diarrhœa set in, and the patient died in February, 1852

The original primary tumour consisted of two large sacs, with irregular prolongations attached to each side of the amputated part of the shoulder blade, and communicating through it The outer sac of about two inches in its greatest diameter, was attached to the bone by a somewhat narrow base, it possessed a thick, strong, white fibrous coat, intimately united to the surrounding parts, (muscles, &c) The inner sac, of about the same size as the outer, had also a narrow base, and a strong fibrous covering, likewise united to the soft parts The inner surface of the first sac was covered with a thick, soft transparent cartilaginous layer, in some places smooth and shining, in other parts rough velvety, and beset with small elevations There were also numerous tuft like formations, of the most various sizes, and lastly, large white fibrous masses, incomplete partitions, and cords Small bony particles were found in portions of the wall At the base of the tumour, the bone itself was in parts rough and uncovered, without any investing membrane, and presenting gelatino cartilaginous masses, similar to those on the wall of the sac On the posterior part of the tumour, the wall was very thick and dense, and on section exhibited a remarkable alveolar structure, containing several cavities, with numerous tufts and projections springing from their walls Many of these cavities were entirely shut and isolated, some communicated with each other by fine passages, others through large openings in their walls, and from this Virchow concludes *that there can be no doubt that the large sac like cavity resulted from the confluence of numerous smaller cavities, produced by the breaking down of their*

---

* Virchow s Archiv, Band iv p 10; Band v Heft ii p 216

walls. From a minute examination of the bone and periosteum in and about the base of the tumour, he is of opinion *that it originated in the development of numerous small foci as well in the bone as in the periosteum.*

Taking into consideration all the pathological data of this case, Virchow believes that the process of development of this class of growths is identical with that already observed by him in *ovarian colloid.* Numerous small alveoli are first formed, consisting of a dense fibrous capsule, and solid gelatinous contents; these alveoli enlarge by increase of their contents, the centre becomes softened, melts into a *mucous* mass, and a cyst is formed. When many such cysts exist, their walls become thickened, break at last in one or more places, and the cysts thus become confluent. New cysts spring up around, and the old ones constantly enlarge, and thus a compound cystoid tumour is formed, with one single or many large sacs, which, however, may have a single appearance.

The first relapsing tumour was seated entirely in the soft parts, with which it was most intimately united, and showed no trace of bone; its wall was thick, white, and covered with a transparent gelatino cartilaginous layer. The relapse of February, 1850, was distinguished from the above chiefly by the existence of numerous small cysts in the soft parts; some isolated, some grouped and confluent.

The necropsy showed most extensive gangrenous destruction of the posterior and lateral parts of the left half of the thorax. The scapula was but little affected above the line of the spine; the portion that remained of the infra spinous space was very uneven, nodulated, and in some parts deeply excavated, and much thinned. At the root of the spine, the bone was destroyed by a cystoid formation, such as those already described, which sprang from it. From the borders of the scapula there extended on all sides numerous cystoid growths—some very large, some entirely isolated, some pressed together and confluent; a large group of them filled the situation of the old cicatrix, and extended backwards to the spinal column; another chain of tumours five to six inches long, two and a half inches broad, extended through the axilla into the neck; a third group was traced through the axilla to the upper arm. About an inch below the head of the humerus there was found an exostosis which formed the base of a large cystoid tumour, whose inner wall was directly in connexion with the periosteum. No such formations were to be found in the ribs or vertebræ.

Virchow examined the growth for the first time on the occasion of the third operation. Some ounces of fluid obtained by puncture presented a reddish brown colour, and it was interspersed with numerous small gelatinous, transparent bodies, so that the entire had the appearance of a red wine soup, mixed with sago. The single gelatinous bodies consisted of a clear hyaline basic substance, in which were deposited cells, in part single, in part aggregated together; many of them pale, grey, granular, and nucleated; others clearer, with finer and larger included fat molecules, besides a nucleus; in short, observes Virchow, nothing essential was wanted in the resemblance to young cartilage. The fluid in which these bodies swam consisted of a clear substance in which similar cells floated, some of them being shrunken and altered in various ways. On the addition of acetic acid to this fluid, a thread like, fibrous, striated precipitate was produced, insoluble in excess of the reagent. Nitric acid produced a precipitate soluble in excess. Alcohol produced a coagulum, soluble in water. Virchow considers this substance to be identical with the gelatinous mass he had before observed in the umbilical cord, and in a tumour in the horse, which he denominates mucin. Scherer confirmed its identity with the substance found by him in the fluid contents of a large cyst in the neck, which he calls *fluid mucin* (Schleimstoff). The tumour itself consisted of a substance which in structure resembled cartilage. Under the microscope it presented an abundant, completely hyaline, intercellular substance, and large thick walled transparent spaces with endogenous elements. On its inner surface, the mass had passed into a thick pulp, having all the character of *fluid mucin* (Scherer). Externally, the intercellular substance was fibrous; the spaces had disappeared, and there remained a thick, fibrous, areolar tissue. *The hyaline inter-*

cellular substance gave no chondrium on boiling, but comported itself as a protein compound Virchow observes that this is the first instance in which a substance has been found in the human body *similar in morphological characters to cartilage, but totally different in chemical composition*

Thus within each sac there was a gelatinous substance morphologically identical with cartilage, and a *mucous* fluid mixed with fragments of this substance Followed in a direction from without inwards, the mass was observed to become gradually somewhat softer, a lightly granular turbid appearance prevailed, which was rendered darker by acetic acid, while the cells became irregular, withered, ragged, and split up In other places, the intermediate substance showed a fibrous, irregular network, or a simply striated appearance, at last the cells and intercellular substance became broken down into the *mucous* fluid At the time these observations were made, Virchow had not yet prosecuted his researches into the nature of the histological elements of areolar tissue He had recognised the accordance between the intercellular substance of cartilage and the areolar tissue substance but he did not know, what he believes he has since established—namely, that the areolar (connecting) tissue possesses permanent elements having a cellular signification * He now considers the point of departure of the disease to be in the areolar tissue corpuscles, and regards the whole cystoid formation as the result of a process of endogenous growth from this starting point The connecting tissue corpuscles are the proper foci of the process, which next determines the development of a quantity of cartilaginous elements The cartilaginous formation is thus the derivative of the same elements of the connecting tissue, which till recently were known under the name of nucleated fibres The genesis of the cystoid thus takes place in the following way, according to Virchow s theory The corpuscles of connecting tissue, scarcely still recognisable as cells, and in which at first long single nuclei are observable, become enlarged, and show themselves again as manifest cells in the interspaces of the intercellular substance, being in some instances filled with fatty molecules, in others with pigment granules While the intercellular substance becomes softer, the cells enlarge, become broader, their nuclei begin to grow, and in some points form long rounded masses, with numerous nuclei This growth of nuclei is followed by endogenous cell formations, and thus is commenced a manifest alveolar type of formation by the grouping together of endogenous nuclei and cells in the interspaces of the compressed areolar tissue The greatest difficulty attends the question of the formation of the hyaline intercellular cartilaginous substance By most especial care, Virchow states, that he was once enabled to find an element which he had already described and figured,† in his essay on endogenous cell formations in cancer These elements are large round or oval bodies, formed externally of a hyaline capsule, analogous to the intercellular substance, they have an internal cavity with endogenous cells A figure is given of one of these bodies (t ii fig 4), with two included spaces, within each of which, but separated by an interval from the edge of the capsule, is seen a shrunken cell, with a nucleus and some fat granules This appears also to indicate the development of the later intercellular substance in the interior of pre existing cells, in the same manner as in the germinal spaces in cancer, cancroid, and cartilage, as already supposed by Virchow He has likewise observed that the gradual change of the fibrous basic substance of the tissue takes place so slowly that a partial inclusion of the former in the latter often occurs He thus thinks that he has been able to trace the history of the development of this peculiar tumour through all its most important phases without interruption, showing the continuity of the growth with pre existing normal elements, even to the highest stage of cystoid degeneration He considers that it is most important that we should not refer the stage of exudation and of free cell formation to an interstitial blastema, and that only endogenous proliferation, growth of constituent parts from a parenchymatous exudation received into tissue elements, and also from contained blastema, should be accepted He believes that these principles will be found applicable to a large number of analogous new formations—in particular to cancerous, sarcomatous, and tuberculous growths, and that these structures, how heterologous soever they may appear, are to be referred to proliferous growth of normal tissue elements, in particular to that of the areolar tissue corpuscles. He enters at much length into the claims of this tissue to be ranked with any known forms of disease, and concludes it to be a particular variety of enchondroma. The entire of this valuable memoir deserves special study.

---

* See Annals of Micrology Part I in our April number

† Archiv Band iii p 213, Taf ii figs 2—4

*Cancer Eburné.**—The affection of the breast so called by Alibert, is but little known to the generality of practitioners. M. Lesauvage has recently observed several instances of it, and has published a memoir with the results of his researches.

This waxy degeneration is in no way allied to cancer; it is extremely indolent, and causes no suffering for a long period. Ledian had already suspected that the peculiar transformation of the areolar tissue of the breast and axilla was continued into that of the organs within the chest. This opinion was confirmed in two of M. Lesauvage's cases, in which he was able to follow the extension of the disease. The areolar tissue between the tumour and the pectoral walls tore readily into dry laminæ, of a dull white colour. The areolar tissue under the pleura also, was indurated and white, in one case, two to five millimetres thick. The serous membranes themselves had undergone no change. It is much to be regretted that no details of a microscopic investigation of this disease (if any such were made) accompany this paper.

Chloroma.—A remarkable case of this peculiar form of disease has been observed by Dr. A. King.† The tumour—which presented itself in a young girl, previously robust, healthy, and free from hereditary disease—appeared first in the temporal fossæ, and then caused the protrusion of both eyes; small tumours of various dimensions presented themselves on the crown of the head, and on the mastoid processes. The appearances post mortem were as follow:—On withdrawing the scalp, the crown of the head was found nodulated all over with flattened swellings, of a yellowish green colour; there were some seven or eight patches, of from two to three inches in their longest diameter, and from half an inch to two inches in their shortest. On dividing the temporal aponeurosis, a diffuse swelling of exactly similar appearance was found to girt the temporal fossæ on each side. No trace of temporal muscles could be detected with the naked eye, but their position was occupied from origin to insertion by the peculiar green substance. The contents of the orbits were converted into the same green substance, with the exception of the eye and its muscles and nerves. The bones were more or less affected. A careful search was made for similar tumours over all the bones of the body, but only two were found, placed symmetrically on the external lateral aspects of the inferior maxilla; they had precisely the appearance of those already described, and the same relation existed between the green mass and the substance of the bone. On the inside of the dura mater there were two flattened masses, placed symmetrically on each side of the falx, projecting inwards and indenting the substance of the brain over the upper and back part of the middle lobes; one of these masses was half an inch in thickness, and felt firm under the knife. The cut surfaces had everywhere the same yellowish green colour; the outer surface of the dura mater was quite healthy. On a careful inspection of the parts removed it was ascertained that the diseased masses presented the same characters in all situations, except in so far as variations were produced by intermixture with the surrounding fibrous, osseous, or muscular tissues. The prevalent colour was a uniform greenish yellow; the masses were in general perfectly homogeneous like size or coagulated albumen, and no bloodvessels or other traces of a complex organized structure could be discovered, either with the naked eye or with a lens. In some situations it was found that the periosteum was converted into the yellow green substance, the bone underneath, however, remaining smooth and natural When the tumours were thickest, spicula of bone were developed, and in all situations the connexion between the spicula of bone and the diseased soft parts was most intimate, and it was only after several months maceration that they could be torn from each other. The microscopic examination of the green substance did not afford very satisfactory evidence of its nature The histological characters agree pretty much with those given by Vogel The substance of the tumours, when altogether free from admixture with surrounding structure, presented a mass of irregular granules, some round, others elongated these were mingled with imperfect fibres or fine filaments, in many situations there was a disposition of the granular mass to a larger fibrous arrangement The larger fibres seem to communicate with each other, or to subdivide and reunite, but had none of the characters of bloodvessels, appear to be quite solid, and split up into smaller fibrillæ From these fibres, probably elements of a fibrous tumour in a state of formation, there were to be distinguished in some parts of the tumour those of fully formed areolar tissue No microscopic appearance was observed capable of explaining the source of the colour Dr R D Thomson reports that he could detect no trace of bile, "the only source from which green colouring matter could emanate in the human system, so far as we know at present" Lebert and Vogel, as is well known, ascribe the colour of this class of tumours to some new proximate principle different from bile, to which the former assigns the name of *Xanthose* Dr King thinks it possible that the cyto blastema consisted simply of extravasated blood, or of blood and lymph During the progress of the case, remarkable changes in volume had been observed, thus two very large exudations at one time covered the mastoid processes, but only one of them could be found after death

---

* Gazette Medicale de Paris, No. 6, 1853.

† Edinburgh Monthly Journal, April, 1853.

Non cancerous, so called Cancroid Formations —It is not our purpose to go at large into this most important subject on the present occasion Some of the chief topics connected herewith have been discussed in recent numbers of this journal * and we need therefore only refer our readers to these sources While so much is being now done to separate growths hitherto confounded together under the common vague term of ' malignant ' to which no precise or definite notion whatever is attached, we regret extremely that any observers of authority should, by the retention and use of names most unworthy of the present *positive* efforts in this department of science, still perpetuate doubt and uncertainty We allude at present more particularly to the work of Mr Paget, and in expressing a difference of opinion with that distinguished pathologist, we do so with great diffidence indeed, and with entire respect for his scientific scruples But we are strongly of opinion that the use of such words as "epithelial cancer" is calculated to retard the progress of accurate inquiry and scientific diagnosis We believe that provisional classification and division, even though further remark should prove them unfounded, lead in every case to accurate individual study '

Epithelioma —We will refer our readers to a review of Hannover's treatise on this subject,† merely remarking that further observation only leads us more strongly to the conclusion, that we are quite warranted in accepting this term to designate a special class of growths now rescued from the indefinite regions of "*cancroid* ' Even Mr Paget's own description of the disease is, we conceive, per se sufficient to condemn the use of the word *cancer* as he employs it, unless, indeed, he would entirely ignore its claims to any *special* significance, a view the justice of which, perhaps, at a more advanced period of pathological anatomy, we may not ourselves be prepared to deny He says,‡ "The essential anatomical character of the epithelial cancer is, that it is chiefly composed of cells, which bear a general resemblance to those of such tesselated or scaly epithelium as lines the interior of the lips and mouth, and that part of these cells are inserted or infiltrated in the interstices of the proper structures of the skin or other affected part" This description agrees almost verbatim with that of Hannover, and with the experience of Lebert and others, and we may add with our own

---

* Review on Cancer British and Foreign Medico Chirurgical Review, Jan 1853

† Hannover on Epithelioma ibid , &c

‡ Paget's Lectures on Surgical Pathology, vol ii p 413

Cancroid or Epithelial Cancer —Under this head a memoir has been published by M Maisonneuve, but we shall only allude to it, as an abstract has already appeared in our pages His conclusions are very similar to those of Hannover

Polypoid Epithelioma —Of this form of epithelioma, the writer has had an opportunity of examining two specimens taken from the meatus auditorius externus by Mr Wilde One was intensely hard and cartilaginous on section, but besides the presence of a small quantity of fibrous matter, it contained nothing but epithelial scales The second was soft in some parts broken down, and with epithelial scales contained an abundance of pusiform bodies

Cancerous Stroma —Rokitansky* has investigated the development of cancerous stroma his memoir is accompanied by sections, illustrating his views

Papillated Cancer (*Zottenkrebs*) —We shall at present merely allude to the researches of Gerlach and Rokitansky on this subject the latter† has published some results of his observations on the histological characters of this form of growth, which must be considered at length at another time

Cancer —The elaborate work of Lebert‡ has undergone review so recently, that we shall not here enter on the question of diagnosis of cancer M Broca's work has also been considered in the same number

Colloid Cancer —The same indefatigable author, M Lebert, has investigated the subject of colloid cancer in a separate memoir § As the result of an analysis by Wurtz, the following proportions are given —Carbon, 48,09, hydrogen, 7,17 nitrogen, 7,00, oxygen, 37,44 In the small proportion of nitrogen it resembles *chitin* which has as yet been found only in insects crustaceæ, and arachnida The cells measured 0,015—0,02—0,01^{mm}, the nuclei, 0,006—0,008^{mm}, and the nucleoli, 0,002—0,0025^{mm} Some of the cases detailed are interesting, as showing the co existence of other pathological lesions

In case 2 there was colloid of the pylorus, with tubercles in the lungs The cells were not very characteristic In case 3 there was colloid of one mammæ, with scirrhus of the other In the fourth case colloid existed in the sigmoid flexure, the peritoneal coat, the colon, and mesenteric glands In this specimen there existed large mother cells, having a diameter of $\frac{1}{20}$ — $\frac{1}{10}$^{mm}, and even in some cases $\frac{1}{5}$^{mm} There was a second class of cells more numerous, having a diameter of 0,04^{mm}—0,05^{mm} There were some characteristic cancer cells in the mesenteric glands In case 6, colloid and encephaloid were combined in an orbital tumour Case 11 was an example of colloid of the uterus and pelvis, with encephaloid of the vagina and bladder The following represents the order of frequency in which parts are affected with colloid, according to this author's experience Stomach (right side), intestinal tube, especially the points of transit from the small to the large, and from the latter to the rectum, the rectum itself, the bones, the glands including the mamma This affection, he considers, must be grouped amongst the cancerous diseases It has usually a long course, and is long localized Rokitansky* also has investigated the nature and structure of colloid growths, his views must be studied at length in his memoir

---

* Sitzungsbericht der math nat Classe d k Akad Wien Band viii † Ibid

‡ See British and Foreign Medico Chirurgical Review, Jan 1853

§ Virchow's Archives, Band iv 1852

Cancerous Phlebitis—Meyer† of Zurich records some observations on this subject, in which he gives a *resume* of the cases already recorded Hasse has given a case of cancer of the liver, in which the branches of the vena portæ and the hepatic veins, as far as the vena cava, were filled with cancerous matter In another case of cancer of the glands of the neck, cancer was found in the subclavian vein, and could be traced thence through the vena cava superior to the right auricle In a third, the cancerous mass was found in the pulmonary veins, and thence passed to the left ventricle Rokitansky states, that in cases of cancer of the uterus he has found cancerous matter in the internal spermatic vein, the hypogastric vein, the internal iliac and crural veins The great question to be solved here evidently is, whether the cancerous matter directly obtained entrance into the veins, or whether the mass within the veins may not have arisen from a cancerous transformation of a coagulum, the result of a phlebitis This latter view, in the opinion of Meyer, contains nothing extraordinary as we see that intra vascular exudation is capable of assuming different forms of organization, being changed to pus, to areolar tissue plates, and fibrous bands, why therefore he asks, may it not, under suitable conditions, undergo cancerous organization?

From a critical examination of the data furnished by a remarkable case given by Virchow, he concludes that in like manner it, too, must be considered as an example of cancerous phlebitis The author himself details a case of cancer of the pylorus, with co existent deposits in the liver in which he was able to trace cancerous matter through the vena portæ and its branches The greater part of the inner surface of the venous membrane was smooth, but in many places it appeared rough and the mucus adhered to it Microscopic examination proved the presence of cancer elements in the contents of the veins

While we must admit that there is no good reason for denying the possibility of cancerous development in the blood *within* vessels we will only remark that these researches of Meyer *in no way prove* that the cancer found in the vessels was thus produced It may possibly have entered their walls by some morbid opening

A very excellent *resume* of the state of science in regard to the diagnosis of cancer by the study of its histological elements, has been published by Dr F Donaldson He will be found on the whole, a supporter of the views of the most advanced microscopists of the day, believing within rational limits, in the specific and diagnostic characters of the nuclei and cells of cancer

A paper by Dr Murchison‡ contains the particulars of an important case of cancer of the uterus and adjacent organs, in which there was also a lesion of the brain much resembling to the naked eye some forms of cerebral softening, but which, on careful microscopic examination, was ascertained to be cancer As in another case published by Dr Redfern,§ there was no fibrous structure present, from which it appears that this element is in no way to be regarded as an essential constituent of true cancer

Tubercle—As with the subject of cancer, so also with tubercle We refer to the analytic reviews of the researches of Virchow, Shroeder van der Kolk, Ancell, &c, which have appeared in late numbers of this journal ||

Tuberculosis of the Vagina—Virchow¶ records an instance of this affection, which he states to have been unknown to him before The right kidney, ureter, and the bladder, presented tubercular patches, some grey and solid, others white

---

* Sitzungsbericht der math nat Classe d k Akad Wien Band ix

† Henle und Pfeuffer Zeitschrift N F Band iii Heft

‡ Edinburgh Physiological Society May 1852 Aug 1852

§ American Journal of the Medical Sciences Jan 18 3

|| See No 21 (Jan 1853 p 183) No 23 (July 1853 p 197) and the present number p 443

¶ Virchow s Archiv Band v Heft iii p 404 and ulcerated, the urethra also was beset with numerous small single, grey tubercles. In the vagina were found similar appearances, in many places the tubercles were grouped in masses on a reddened base, the single ones resembled fine grey pearls. The microscopic appearances were similar to those of tubercle, presenting the same soft cellular nucleated masses which this author considers to indicate young tubercle. The uterus, rectum, and left kidney, contained only some cysts.

Scabies Crustosa (Spedalskhed) Sc. Norwegica Boeckii.—The labours of Danielssen and Boeck have made this form of tubercular lepra sufficiently well known. In a case remarkable for the thickness of the yellowish brain crusts, which were as hard as horn, they found by the aid of the microscope millions of dead acari in the tubercles, on the surface as well as in the softened ulcerated substance. Three authors were not clear whether or not this animalcule was to be ranked with the sarcoptes scabiei. Fuchs of Gottingen* has met with a case in a patient 42 years of age, who stated that he had suffered from the disease since his youth. Under the microscope, the mass showed quantities of epithelial scales, overlying each other, between these, as in Boeck's case, there were innumerable itch mites, of all sizes and sexes, with their young, ova, and excrement. Many of these acari were alive, and moved with activity, they resembled completely the usual sarcoptes. This case, remarks the author, establishes that the *scabies crustosa Boeckii* is not peculiar to Norway, and he is further of opinion, that the animalcule is the same variety of acarus in the sarcoptes scabiei.

Complete Stoppage of the Œsophagus by Aphthous Masses.—Virchow† gives the particulars of the case of a child seven weeks old, in which the œsophagus was completely blocked up by a solid cylinder of aphthous product. On transverse section, there could not be found any trace of a central canal, the mass reached as far as the cardiac orifice. Under the microscope it showed the well known numerous threads and spaces, with remains of ingesta, milk, epithelial formations, &c. In the lungs, also, some masses were found.

ANATOMY, PHYSIOLOGY, AND ORGANIC CHEMISTRY.

The Determination of Urea. By Professor Liebig.

The new method proposed by Liebig has the advantage of simultaneously determining the amount of chlorine and of urea. It is as follows:—100 grammes‡ of mercury, carefully purified from bismuth and lead, are dissolved in pure nitric acid, the salt is evaporated to a syrup like consistence, and then enough water is added to reach accurately to 1400 cubic centimetres. Every 100 cubic centimetres contains 7.140 grammes of mercury. If this solution be added to a solution of pure urea a *snow white* precipitate falls, which is a compound of urea and oxide of mercury $(U + 4HgO)$. When the urea is all precipitated, and when, consequently, nitrate of mercury is in the solution, a *yellow* precipitate, hydrated oxide, is thrown down by the addition of a little carbonate of soda to a drop of the urea. In order to determine the exact moment when the yellow precipitate occurs, the solution of the nitrate of mercury is of course added drop by drop from a burette, and from time to time a drop of the urine is taken and tested with the soda. One cubic centimetre of the solution corresponds to 10 milligrammes of urea, and from the quantity used, the amount of urea is calculated.

---

* Henle und Pfeuffer Zeitschrift N. F. Band iii. Heft ii.

† Verhandlungen der Phys. Med. Gesellschaft z. Wurzburgh. Band iii. Heft iii. 1852.

‡ We have retained the French weights as their conversion into the inconvenient English weights is difficult, and as these delicate manipulations will not permit fractions to be disregarded.

Although nitrate of mercury will thus precipitate urea, the bichloride will not do so, and on this fact is founded the determination of the chlorine If the solution of urea be not pure, but mixed with chloride of sodium, no precipitate at first occurs with the nitrate of mercury, because sublimate is formed, if the nitrate continue to be added, then at last, the chloride being exhausted, the mercury combines with urea, as in the pure solution Therefore the quantity of solution used before the white precipitate appears, shows the amount of chlorine which must have combined with the mercury first of all A cubic centimetre of the mercurial solution corresponds to 10 milligrammes of chloride of sodium

In testing urine the phosphoric and sulphuric acids are first precipitated with barytic water, and after filtration, the urine is weakly acidified with nitric acid

Although both the chlorine and the urea *may* be determined with the *same* quantity of urine, it is advisable to use two portions, one for the chlorine, and one for the urea The quantity of nitrate used for the chlorine alone in the first specimen, can be deducted from that used for the urea and chlorine together in the second, and the remainder gives, of course, the quantity which has been used for the urea —*Annalen d Chem und Pharm*, Band lxxxv 1853

On the Effect of Coffee By Dr ZOBEL

IN a long and interesting article Dr Zobel discusses the effects of coffee as well as, incidentally, other dietetics He denies that the use of coffee (and of tea) is to be estimated by the quantity of nitrogen it contains, and shows by calculation how comparatively small a quantity of nitrogen could by this means enter the system He also denies the accuracy of Rochleder's opinions, that coffee gives rise to the formation of creatine or if it does so, he questions whether this may not result from its action on the nervous system, and not by immediate transformation of its own substance With respect to the influence of coffee on the health, he refers to the opinions of a few enthusiasts, such as Jury and Thierry, who have supposed it to be most prejudical to life He then inquires what are the chemical changes which occur in the caffein when introduced into the blood The first change he states to be as follows

$$\text{1 equiv caffein } C_{16}N_{4}H_{10}O \text{ makes } \begin{cases} \text{1 equiv hydrocyanic acid } C_2NH \\ \text{1 equiv of a body of the composition } C_{11}N_2H_9O \end{cases}$$

The quantity of hydrocyanic acid is very large, and quite enough to appear at first sight a justification of those who have asserted the danger of coffee But if the examination be continued, this apprehension is dissipated, another equivalent of caffein acting on the substance formed by the separation of the prussic acid, gives rise to four equivalents of ammonia, the great antidote of the acid, and a third equivalent of caffein and water gives rise to one equivalent of quinine, two of oil of turpentine, and three of urea, while twenty seven of oxygen remain The formula is thus given —

3 equiv caffein $=3(C_{16}N_4H_{10}O_4)$	are	$C_{48}N_{12}H_{30}O_{12}$
23 equiv water	are	$H_{23}O_{23}$
		$C_{48}N_{12}H_{53}O_{35}$

Thence are formed—

1 equiv hydrocyanic acid	$C_2N_1H_1$
4 equiv ammonia	N_4H_{12}
1 equiv quinine	$C_{20}N_1H_{12}O_2$
2 equiv oil of turpentine	$C_{20}H_{16}$
3 equiv urea	$C_6N_6H_{12}O_6$
27 equiv oxygen	O_{27}
	$C_{48}N_{12}H_{53}O_{35}$

By the influence of the ammonia and turpentine, the sedative effects of the hydrocyanic acid are neutralized, and are replaced by a slight stimulant effect on the peripheric nervous system The ammonia would produce its stimulant effect, and soon pass off, were it not fixed in some measure by the strongest neurine tonic known, the quinine To these effects of caffein, it is necessary to add those of the tannic acid and volatile oils which the roasted coffee nuts contain when coffee itself is drank Dr Zobel thus confirms the statement of Lehmann that the amount of urea is increased by the use of caffein A few pages at the end of the paper are devoted to a consideration of the mode of origin of caffein in the plant, which we omit —*Prag Viertel Jahrsch*, 1853 Band ii pp 105—136

The Effect of Tea By Dr Bocker

Dr Bocker details at great length several series of experiments made on himself In the first he determined the exact amount of food and of water (each of known composition) taken into the system, the exact amount of exercise, the exact amount of urine and its constituents, and of fæces, and of expired carbonic acid The amount of perspiration was calculated These experiments were continued for seven days

In the second series the experiment was conducted exactly in the same manner, only cold tea was substituted for water These experiments were also continued for seven days The following table shows the mean result of the experiments on the weight of the body, the fæces, urine, and perspiration

In 24 hours	1st Series 1260 grammes of water being taken *		2nd Series 1260 grammes of tea being taken	
A Exercise	84 14	minutes	87	minutes
B Excretions,—				
1 Loss of weight	539	grammes	203	gram
2 Quantity of fæces	178 30	,,	96	,,
3 Calculated perspiration	1349 90	,,	1335 7	,,
4 Urine	2621 143	,,	2550 000	,,
Water	2513 519	,,	2474 016	,,
Solids	77 624	,,	75 984	,,
Urea	35 194	,,	34 221	,,
Uric acid	0 356	,,	0 231	,,
Ammonia	0 421	,,	0 660	,,
Muriate of ammonia	1 250	,,	1 959	,,
Oxalic acid	0 092	,,	0 088	,,
Potash	4 466	,,	5 274	,,
Sulphuric acid	2 841	,,	2 815	,,
Sulphate of potash	6 187	,,	6 132	,,
Chloride of potassium	1 774	,,	3 113	,,
Chlorine	11 175	,,	10 687	,,
Chloride of sodium	16 156	,,	13 043	,,
Phosphoric acid	2 923	,,	2 926	,,
Phosphate of soda	5 483	,,	5 488	,,
,, lime	0 724	,,	0 772	,,
,, magnesia	0 756	,,	0 717	,,
Salts by incineration	28 633	,,	27 229	,,
Volatile salts and extractives	13 309	,,	14 304	,,
C Food—total weight	3610 50	,,	3617 00	,,
Water	2938 84	,,	2957 87	,,
Solids	671 66	,,	659 13	,,

* We have retained the French weights

When the tea was taken, it appears that not only was more nitrogen introduced by it, but that daily three grammes of dried meat was also taken in excess of that consumed during the series of experiments with water, and yet, in spite of this greater quantity, the average amount of fæces, and the amount of urine, was diminished by the use of the tea It would then seem that (contrary to Lehmann's statement) the quantity of urea (judging only from these experiments) was *not* augmented, but, on the contrary, both the *urea and uric acid were diminished* The author confirms these results by another series of experiments, in which the ordinary amount of food was taken the quantity of urea was much greater than with the use of tea

We need not quote the average table of the carbonic acid, but state merely the result—viz, ' that tea, in the quantity mentioned, exerts no marked influence on the quantity of carbonic acid excreted, or on the frequency of the respiration, or on the beats of the pulse "

The author demands whether the diminution of excretions observed from the use of tea, might not be accidental—i e, attributable, not to the tea, but to the unexplained variations in the metamorphosis of tissue He answers this in the negative, because he has found by previous experiments that in his own case these variations occur at stated times of the year, and that during the period of his experiments he has no doubt the amount of excretion would, under the usual conditions, have remained stationary —*Archiv des Ver, fur gem Arbeit*, Band i Heft 2, p 213

On the Constituents of the Flesh fluid of Frogs By Dr GROBE

PROFESSOR MOLESCHOTT has affirmed that both urea and oxalic acid exist in the fluid which can be expressed from the muscles of frogs To test the accuracy of this statement, Dr Grobé has undertaken an elaborate investigation in the laboratory at Giessen, which has led to the conclusions—

1 That neither urea nor oxalic acid exist in this fluid
2 That the crystals considered to be these substances were creatin, creatinin, and sulphate of potash
3 That the same organic and inorganic substances are found in the flesh of frogs as of other animals —*Schmidt Jahrb*, Band lxxviii No 6

On the Action of Water By Dr FALCK

THE author has endeavoured to ascertain to what extent the imbibition of pure water, at a temperature of 60° Fahr, increased the excretion of the water and the solids of the urine The following table (in which we have retained the continental weights) shows the result at a glance

—	At the commencement of each period of 12 hours he took—			In each 12 hours he excreted—					
	Bread in grammes.	Pudding in grammes	Water in grammes	Urine in grammes	Solids of urine in grammes	Water of urine in grammes	Specific gravity	Solids per cent	Water per cent
1	500	164		332	19 568	312 432	1 030	5 894	94 106
2	500	161	1000	489	27 184	461 817	1 027	5 559	94 441
3	500	165	2000	1310	27 243	1283 757	1 008	2 078	97 922
4	500	165	4000	3911	33 478	3877 522	1 003	0 856	99 144

If the experiments 2, 3, 4, are compared, it will be seen that as the quantity of water drank was augmented in geometric progression with the factor 2, so the quantity of water passed in the urine augmented in geometric progression with the factor 3 or in other words if the quantity of water drank was doubled, the quantity of water passed was trebled The geometric progression is visible also in the specific gravities, and (inversely) in the per centage of the solids, so that, in fact, the calculation based upon the geometric progression is almost identical with the results actually determined by experiment

These experiments show that the quantity of solids carried out of the body does not increase with the quantity of fluid drank One pint of water washes as much organic detritus out of the body as two or four pints, temperature and other circumstances being of course constant [This result appears completely opposed to the experiments of Becquerel, on which much stress has been laid by the advocates of the water cure —Ed]—*Vierordt's Archiv*, 1853, Heft 1, p 150

PATHOLOGY AND PRACTICE OF MEDICINE

On the condition of the Grey Substance of the Brain after excessive Mental Exertion By Dr Albers

In allusion to a statement made in the 'Psychological Journal' by Dr F Winslow, in an article entitled "The Overworked Mind," that the grey substance undergoes softening as a consequence of excessive mental exertion—Albers states that he has dissected the brains of several persons, who have for many years undergone great mental labour, and that in all of these he has found the cerebral substance unusually firm, the grey substance as well as the convolutions being remarkably developed In several of these instances a settled melancholia had taken possession of the mind during the latter period of life He believes therefore, that to produce a softened condition some additional influence beyond mere over exertion is required Softening of the cortical substance is a frequent consequence of apoplexy of the convolutions, which gives rise to numerous small depositions of blood, especially at the convex portions of the brain being accompanied also by an atheromatous degeneration of the small arteries In this latter condition the quantity of fat is not only accumulated in the arteries, but also in the cerebral substance itself This degeneration is oftenest seen in gouty subjects, in whom it certainly is not attributable to excess of mental exertion Several such cases, too, have been met with in rustic labourers —*Froriep's Tagsberichte*, No 696

On the Secretion of Sugar by the Liver, and the Modification of this by Disease By M Vernois

M Vernois, with the object of verifying and extending M Bernard's researches, has submitted 173 livers to examination, and the following are the general results he has arrived at —1 He believes with Bernard, that sugar constantly exists in the human liver, independently of alimentation He found it in a case in which no food had been taken for fifteen days 2 *Age* appears to exert a certain amount of influence The minimum amount was found from birth to two years of age the maximum from twenty to fifty years of age Sugar is found in the liver of the fœtus (as early as the fourth month) but in less quantities than in that of the adult There may be sugar found in the liver of the fœtus and not in that of the mother, and *vice versâ* 3 *Sex* exerts no appreciable effect 4 The *influence of disease* upon the secretions is indubitable (1) The quantity of sugar is in an inverse ratio to the *duration* of the disease (2) The *nature* of the disease which has caused death influences the secretion In some diseases, and especially in epidemic cholera, sugar is more easily found in the liver than in the normal state while in the majority of diseases sugar is seldomer detected, as in sclerema, cerebral affections, tubercular disease, &c. (3.) The *anatomical conditions of the liver* exert an influence, the quantity being lessened in proportion as the secretory structure is destroyed. The putrefactive process, however, does not prevent its detection. Gubler says none is found in the livers of syphilitic infants. 5. The opaline aspect of a decoction of the liver is usually proportionate to the amount of sugar, as determined by the potash and Trommer's tests.—*Archiv Gén.*, 1853, i. p. 657.

On Intermittent Pneumonia. By Dr. CONSTANT.

DR. CONSTANT, practising in one of the marshy districts of the department of the Lot, draws attention to the signs which distinguish what he terms intermittent pneumonia, as when they are overlooked the disease proves rapidly fatal.

1. The initial shivering is usually more intense and prolonged than in ordinary pneumonia. 2. The pleuritic pain is felt early and always in front of the chest, although the pulmonary congestion is almost always localized posteriorly. It is much more amenable to blisters than to leeches. 3. Violent cephalalgia is one of the earliest symptoms, being either frontal or sincipital, and it is often accompanied by severe lumbar pain, which observes the same stages of increase and decrease as itself. 4. The shivering is followed by intense heat, which after several hours gives place to abundant sweating. 5. The pulse, during the paroxysm, in place of being full, strong, and vibrating, as in ordinary pneumonia, is rapid, soft, undulant and compressible. 6. There is never any purulent expectoration, these pneumonias never proceeding beyond the second stage—i. e., red hepatization, the pulmonary engorgement being rather a sanguineous congestion than inflammation. 7. Auscultation and percussion are of the highest value, often revealing the disease when unsuspected. A distinctive feature is the rapid passage from the first to the second stage of the disease, so that 8 or 12 hours after auscultation had revealed only a slight circumscribed *râle*, a whole side will be found hepatized. Under the influence of large doses of quinine this rapidly disappears, giving way to returning subcrepitant *râle* during the remission of the fever but returning again during the paroxysm if this have not been cut short. 8. The crepitant *râle* of the first stage is almost always moist, the parchment crackling *râle* only having been heard for a short period two or three times in more than 60 cases. It invades large surfaces rapidly, being heard posteriorly, sometimes laterally, but never in front. 9. This form of pneumonia especially affects the posterior part of the lower lobes. 10. It especially appears in summer and autumn, while ordinary pneumonia prevails in spring and winter. 11. It attacks all ages indiscriminately except early infancy. 12. The blood which flows from a vein is often below the normal temperature, very black, and deficient in plasticity. After rest, its surface acquires a bluish colour, especially if the patient is taking quinine. The clot is slow in formation, and soft. The buffy coat is absent, or very thin, and inclines to a bluish colour. This condition of the blood, conjoined with the soft pulse and rapid hepatization, constitutes the chief distinctive sign of the affection.

In this district, during winter, purely inflammatory pneumonia is met with, but in proportion to the high temperature and the production of malarial emanations, this inflammatory element is replaced by the paludal one. There are indeed three forms met with:—1. Simple pneumonia. 2. Spring inflammatory pneumonia, complicated with the intermittent paroxysm. 3. Summer and autumn intermittent pneumonia. The first requires bleeding and antimony, the second, antiphlogistic treatment with quinine, given either simultaneously or subsequently, and the third, quinine in combination with external revulsives. These forms may still undergo further admixture, accordingly as the inflammatory or paludal element prevails, requiring appropriate modification in the treatment.—*Bull. de Therap.*, xliii. pp. 451 & 491.

SURGERY.

On the Reduction of Luxations of the Extremities, when complicated with Fracture. By M. Richet.

M. Richet relates, in great detail, the case of a man, æt. 68, in whom a dislocation of the upper end of the humerus was complicated with fracture of the anatomical neck of the bone. Four days after the occurrence of the accident, he was placed under the influence of chloroform, and the reduction of the dislocation was easily effected by pressing backwards the head of the bone, without any traction being resorted to. The fracture was afterwards adjusted and consolidated, and when the patient was seen a year after, he had recovered the complete use of his limb.

M. Richet reviews the opinions of the classic writers, who agree in pronouncing the impossibility of reducing a dislocation of the humerus or femur, until after the fracture complicating it has become united. He shows the great power chloroform confers upon us in these cases, by the complete relaxation of the muscular resistance it produces, and the ease with which the head of the bone may, by due manipulation, be forced back into its socket. He thinks the passive obstruction offered by the fibrous tissues of the parts has been exaggerated and ill understood.—In numerous autopsies he has made after recent dislocations produced accidentally or experimentally, he has always found the aperture in the capsule broad and irregular, and in no condition to offer an obstacle to reduction. He does not deny that such obstacles may occasionally be offered by the fibrous structures, independently of the aperture of the capsule, but he maintains, from clinical and experimental observation, that such obstacles are much more easily overcome by pressing the head backwards than by the usual practice of traction of the limb which, indeed, only aggravates them. By the aid of chloroform, he believes a dislocation of the humerus into the axilla may thus always be reduced by pressing the head directly backwards. In several experiments that he has made, in which the head of the femur has been dislocated, and the bone then sawn through just below it, so as to simulate dislocation complicated by fracture, the reduction has also been easily effected by direct pressure.—*Bulletin de Thérap.*, tom. xlv., pp. 18, 104.

On Traumatic Effusions of Serosity. By M. Morel Lavallée.

The author here draws attention to contusions, in which the subcutaneous effusion has consisted, not of blood, but serosity, and as the subject is quite unnoticed by systematic writers (except incidentally by Velpeau, in his 'Closed Cavities'), he gives a brief history of it, derived from the 12 cases he has collected.

The *causes* in the great majority of cases have been the oblique pressure of a wheel in motion, brought in contact with the part, and by its rapid motion causing extensive detachment of the skin from the adjacent aponeuroses, without inducing any breach in its surface. The pressure of a wheel of a carriage should always raise the presumption of a production of this lesion. As yet the occurrence has only been met with in men, whether from their being more exposed to external violence, or from the greater quantity of adipose tissue under the skin in women rendering its detachment less easy. The *nature* of the lesion seems to consist in the rupture of minute or capillary vessels, the bruised extremities of which allow only the thinnest portion of the blood to escape. It resembles the serous discharge that issues from a wound that no longer bleeds or which is imbibed by the first dressing after amputations. The *effused fluid* is usually abundant, amounting to more than a *litre*, being generally limpid, and of a light citron colour. It may, however, assume a reddish or even a blackish colour, and this has led to its being mistaken for blood prevented from coagulating by the contact of living tissues. It undergoes no coagulation or any perceptible change under the influence of time

Its permanent fluidity quite distinguishes it from blood, as it never coagulates except when mixed with blood simultaneously effused. The examination of its composition by Lebert and Robin leads to its being considered as analogous to the serosity of the blood, containing some fatty globules, and some almost colourless blood corpuscles. On standing, about $\frac{1}{20}$th of its quantity is deposited, consisting of colouring matter in the form of minute granules.

Symptoms.—There is nothing constant in regard to pain. The skin usually preserves its normal colour though occasionally it is ecchymosed. The collection may assume the form of a tumour, but usually it remains flattened and undefined. The fluid is spread out in a large cavity, which only becomes gradually filled, and generally, never indeed, becoming quite full. The commencement of the collection is thus often very obscure, and its amount is much greater than external appearances would lead one to suppose. Owing to the partial fulness of the sac, a tremulous movement, visible to the eye, is produced on the motion of the parts, and sometimes a strong current of air suffices to induce its undulation. To produce true fluctuation, the skin has to be stretched, or the fluid forced into one part of the sac. In most cases, a more or less distinct ridge surrounds the base of the tumour.

Diagnosis.—It has often been mistaken for an effusion of blood, especially when the fluid has been of a dark or red colour; and when it has been accompanied with pain and fever, it has given rise to the belief of suppuration.

Treatment.—All topical applications have failed; and opening the tumour is the only efficacious procedure. Still experience thus far shows, that success is proportionate to the delay with which an aperture is made. In general, M. Morel prefers puncturing the tumour by means of a trocar to subcutaneous incision; though in some cases the latter may be preferable. Compression is to be afterwards employed to induce adhesion of the walls of the sac; and flying blisters will be found of utility. If the fluid is reproduced, the subcutaneous incision may be resorted to, a little charpie being also employed to keep the aperture open. When simple puncture and well regulated compression do not succeed, and the lesion assumes a chronic form, we need not hesitate to employ iodine injections. A remarkable example of the success of this practice is given.—*Archives Générales*, 1853, tom. i. p. 691.

Case of Hydatids of the Breast. By M. MALGAIGNE.

AN example of this exceedingly rare affection came under M. Malgaigne's care, at the St. Louis. It occurred in a woman æt. 42, who had had two children (the youngest being 10 years of age), but had never suckled. She was in good health, and not wasted; and had never received a blow upon, or suffered from pain in, her breasts.

About six years since, she first perceived a tumour of the size of a pea at the inner and lower part of the left breast, and it slowly increased in size, until during the last year, when it has remained stationary. On admission, an oblong, roundish tumour, the size of a pigeon's egg, presented itself, which was moveable over the subjacent tissues, but adherent in part to the skin. The skin was of its normal colour and thickness. The tumour offered a remarkable resistance, and was not at all tender. No fluctuation could be felt, and the axillary glands were unaffected.

When M. Malgaigne made an incision across the tumour, a jet of transparent citron coloured fluid issued, and the tumour at once subsided. On pressing at the sides of the incision, a largish hydatid cavity issued forth. It had been opened by the incision, but still contained some of the fluid. Its walls were whitish and semi transparent; but no vesicle was found in its interior. The serous sac, which had contained the hydatid, adhering to the surrounding tissues, required entire extirpation. The wound healed readily.—*Rev. Med. Chir.*, tom. xiv. p. 55.

Contribution to Ophthalmoscopia By Dr T Czermak

Dr Czermak calls again the attention of the profession to the advantages gained in the examination of the eye by an ophthalmoscope described by the author in 1851 ('Prager Vierteljahrschrift,' vol iv), under the name of "the Orthoscope" The leading idea in the construction of this apparatus was ' to prevent, as much as possible, the refraction and deviation of the rays of light by the anterior surface of the cornea " This intention he realized by placing the eye under water, the refractive power of which is nearly equal to that of the aqueous humour and cornea The water is kept before the eye by means of a little case with a transparent anterior wall Through this the eye becomes extremely far sighted (*weitsichtig*), the foci of the objects are more or less thrown behind the retina, according to their distance, large circles of diffraction (*zerstreuungs kreise*) are formed on the retina, the anterior focus becomes removed from the eye, it is only in their passage through the crystalline lens that the rays reflected from within undergo a considerable deviation from their original direction In this way the conditions by which the inspection of the retina is ordinarily prevented, are so far changed, that by means of a good light (concentrated day as well as candle light) the whole interior of the eye may be easily looked at Czermak remarks, that already in the last century this fact was well known ('Le Cat, traité des sens,' Amsterdam, 1744, p 174) but was scarcely introduced into practice As an important advantage of the Orthoscope Czermak considers also the circumstance, that the reflected little image of the candle flame on the cornea, which frequently is so troublesome in the examination with the instruments hitherto used, is observed only in a slight degree on the anterior wall of the apparatus Professor Arlt, who has derived much benefit from the Orthoscope, in his "Klinik,' makes use of a small case of gutta percha (with a glass wall in front), which easily fits to the face, by which the running out of the water is perfectly prevented —*Prager Vierteljahrschrift, f pract, Heilkunde*, 1853, ii 137 ss

---

MIDWIFERY, &c

Anteflexion of the Uterus as a normal condition prior to Pregnancy

By M Boulard

A few years since, a very animated discussion took place at the Académie de Médecine, upon the subject of engorgement of the uterus M Velpeau was one of those who maintained that anteflexions of the uterus are often mistaken for engorgements, while other speakers declared anteflexion itself to be a very rare occurrence M Boulard, engaged in searching for examples of the deviation for Velpeau, was at once struck by the frequency of its occurrence in young subjects, while in older ones he scarcely ever met with a case, without, from the examination of the other organs, ascertaining that the subject of it had never been pregnant After in this way collecting a great number of uteri from the dead house, M Boulard resolved to study the point during the development of the organ, and found that the uterus is almost always anteflexed in the fœtus He has continued to pursue the investigation from that period, and the present paper is founded upon the examination of 27 adult female subjects who had never borne children, 19 young girls from two to thirteen, and 57 full timed fœtuses In 98 of these anteflexion has been found, and to such a point is the body bent upon the neck, that it is not possible to prevent its regaining the same position immediately after the attempt at replacing it has been made Since these observations have been made, the author has sought every opportunity of verifying them in the living subject, but he has only had ten opportunities of examining the position of the uterus in the virgin, and in all of these the anteflexion existed

M Boulard accounts for this disposition having been overlooked by anatomists, by the fact that the bodies of women who have borne one or more children are those which are usually brought for dissection, and the organ is examined only after it has undergone this physiological change, or under the influence of the relaxed state of the tissues after death When anteflexion has been noted, it has been considered as an abnormal and diseased condition, and regarded as a cause of suffering which was due really to some morbid condition which had become accidentally associated with it —*Rev Méd Chir*, xiii 341

On the Effects of Menstruation on the Milk of Nurses

By MM Becquerel & Vernois

Upon this effect, which the occurrence of menstruation exerts in women who are suckling, there is discrepancy of opinion among authors, the majority, however, with the public at large, believing in its deteriorating influence So great is the difficulty in obtaining true statements upon this point, that among the great number of hired nurses in Paris, the authors have only been able to examine the condition of the milk in three women while actually menstruating In these the density of the fluid was found slightly diminished, as was the proportion of sugar, and the proportion of water was sensibly so The solid parts were notably increased, especially the caseum The authors cannot believe that such changes in composition can induce any mischief beyond some temporary derangement in the digestive organs and even this might be prevented by causing the child to suck less, and letting it drink a little sugared water, to replace the sugar and water lost during menstruation

In the discussion that followed reading the paper, M Roger observed, that while attached to the Office for Nurses, he had paid considerable attention to this point, and that he had arrived at the following conclusions —If the menses reappear easily without pain or derangement of the nurse's health, while her milk is under 12 or 15 months old, and the quantity of blood lost is normal and moderate the quantity of milk does not become diminished, or its qualities altered, and the child does not suffer from its use If, however the menses are too abundant or too frequent the milk may diminish in quantity or disappear The same effect is also produced, though more slowly, in some days or weeks, when the menses are prolonged for a week, so that the loss is considerable The milk will much more certainly dry up if the menses reappear at an advanced period of lactation—this being then the signal of the imperfection and approaching termination of the secretion

When the milk becomes thus diminished, it rarely exhibits the physical characters of poor milk, but by its density, whiteness, and the excess in number and size of its globules it more approaches in character and richness cow's milk When the menstrual epochs reappear with difficulty and are attended with pain, indigestion diarrhœa, &c, or are preceded or followed by leucorrhœa the child may suffer symptoms due to indigestion induced by the altered characters of the milk—the alteration of the milk chiefly consisting in increase in the number and size of the globules These influences are, however, only temporary, and the milk soon recovers its normal character The ailments which the child hence suffers are only temporary, and have been greatly exaggerated —*L'Union Médicale*, No 70

Reposition of the Prolapsed Funis By Dr Fincke

The author relates five cases in which the plan he adopts has been attended with success Passing the entire hand into the vagina, and catching the middle of the prolapsed coil upon the points of the fingers, he carries it as high up into the cavity of the uterus as possible, and leaves the funis supported between the posterior wall of the uterus and the parts of the child If, however, he withdrew his hand as he passed it in, he always found the funis follow it, but by doing so in another manner, he has found it continues where he placed it. After detaching the funis, the hand, placed at the upper and posterior part of the uterus is held horizontally, and withdrawn while describing about the fourth part of a circle forwards, and then bringing it straight downwards. If the funis follows it, it will have twice to form a right angle; but in his cases it has hitherto not done so, and the children have been born alive.—*Monatsbericht fur Geburtskunde,* Band i. 437.

Stricture of the Fundus Uteri as a cause of dangerous Hæmorrhage.

By Dr. Diez.

UNDER this new title the author alludes to a description of hæmorrhage that most experienced practitioners are familiar with as arising from partial contraction of the uterus. After the separation of the placenta, and while globular contraction of the uterus can be felt above the pubes, alarming hæmorrhage comes on. As there is good contraction of the uterus, and pains, often of considerable severity, occur, this is at first inexplicable. On passing the hand through the widely opened os, we enter a tolerably spacious cavity filled with coagula, whose walls are firm and unyielding, and remain quite passive to the pressure of the hand. Pushing on farther, we perceive a more or less narrow, often cartilage-like stricture, separating the fundus from the body of the uterus. It is exactly the same as is observed in strong incarceration of the placenta, only, as a general rule, the contracted part is inclined to one side (usually the right), and takes on a more oval form.

The means calculated to check hæmorrhage under other circumstances are here useless or mischievous. Increasing the pains by means of ergot, cold, external pressure, friction, &c., does injury by increasing the abnormal contraction, and by favouring the flow of blood from the non-contracted part. Of a more useful character are such means as tend to arrest hæmorrhage without exciting pain, as compression of the aorta (taking care, however, not to compress the fundus uteri), acids, alum, cinnamon, ipecacuanha, warm astringent injections, &c. The most efficacious means however, is the attacking the pathological condition upon which the hæmorrhage depends by the employment of belladonna, in conjunction with, or in quick succession with, ipecacuanha, together with compression of the aorta. When, on account of pressing danger, the operation of these means cannot be waited for, the painful and often difficult procedure of dilating the strictured part must be resorted to, allowing the hand to remain within the fundus until the organ contracts in a normal manner.—*Monatsbericht fur Geburtskunde,* Band ii. p. 1.

THERAPEUTICAL RECORD.

Aneurism.—In a case of false aneurism at the bend of the arm following bleeding, M. Velpeau (L'Union Med., Aout 25) employed the method of Pravaz, and injected 8 drops of perchloride of iron into the sac; the blood did not appear to be coagulated. Twenty days afterwards, 8 drops more were injected, without producing coagulation, and six days subsequently the brachial artery was tied.

In a case of traumatic false aneurism of the subclavian artery, M. Bonnet (Archiv. Gen., Aout) being unable to ligature, and unwilling to use galvano-puncture, applied over the tumour the chloride of zinc and produced an eschar, after removing the superficial layer of which he re-applied the caustic, and so proceeded for five weeks. He presumed that the chloride of zinc would penetrate into the sac, and cause coagulation of the blood. About the fortieth day the pulsation and the murmurs had completely disappeared.

Asthma Spasmodic —M Trousseau (Gaz des Hop, No 93) observes, that although the *modus operandi* is difficult of explanation, he has relieved many patients during the paroxysms of asthma, by the old plan of burning in the room paper impregnated with nitre

Cardialgia —See Creosote

Chloroform, Poisoning by —In a case of a strong man, whose testicle was extirpated, Ricord (L'Union Med, 1853, No 34) administered chloroform, narcosis was induced in half a minute, the operation was performed Some little time after the chloroform had been removed from the mouth, the pulse and respiration suddenly ceased, and the face became pale Ricord threw himself on the patient to whose mouth he applied his own mouth, blew strongly into his lungs, then compressed the thorax to expel the air and then again blew into the lungs After two or three of these artificial respirations, the pulse returned In this case, Ricord believes that there was syncope coming on as it will after hæmorrhages, or mental excitement, and that this was cured by the *immediate* use of the artificial respiration

Cholera, Asiatic —Mr Macpherson (Assoc Med Jour, August) has employed cold affusion in India, with good results

Collodion —Two varieties of collodion are recommended by Clarus (Schmidt's Jahrb No 8, p 27), the first for closing wounds, and the second especially for cutaneous eruptions, in which collodion is useful (a) *Collodium terebinthinatum* a scruple of turpentine dissolved in ℥j of collodion, forms a very tough and adhesive fluid (b) *Collodium ricinatum*, a scruple of castor oil and ℥j of collodion forms a thin and soft solution well adapted for the skin

Creosote —M Arendt (Froriep s Tagesberichte, Nos 691 & 627) states, that the great advantage he had derived from the use of creosote in asthma and bronchitis, an account of which he published in 1848, induced him to employ it in various other affections, especially of mucous membranes In *chronic varicose ophthalmia* he found from 1 to 3 drops of creosote to 1 ounce of water a valuable collyrium, dropped into the eye several times daily *Cardialgia*, and especially the idiopathic form in women, was speedily amenable to creosote, 3 drops in sugared water relieving the severest pain, a repetition in two or three hours being rarely required *Leucorrhœa* whether vaginal or uterine even when very obstinate, often yielded in a few days to a lotion of 2 drops to the ounce, thrown in two or three times a day So also three or four injections usually sufficed for the cure of *gleet* In *menorrhagia* in non pregnant women and in some cases of hæmorrhage prior to delivery, due to *placenta prævia*, it has been found very useful Indeed, it is a valuable *hæmostatic agent*, whenever the bleeding proceeds from small vessels, and especially those of mucous membranes In some of these cases a more concentrated mixture is required, as 10 to 20 drops to the ounce

Delirium Tremens —Dr Prat (Ann Ther and Sch Jahrb, 1853, No 8) has administered by the mouth, *chloroform* in considerable doses, in a case related, after three doses sleep came on

Epilepsy —Dr Bucknill (Lancet, August 13) relates two cases of epilepsy in lunatics treated by tracheotomy In the first case, after the operation (thirteen months before date of report) the fits were greatly mitigated in number and severity, in the second case reported, (two months after operation) there was equal benefit from the operation

Dr Brown Sequard (Philadelphia Med Examiner, April, 1853) states that cauterization of the larynx in epilepsy may be substituted for tracheotomy He has, however, only treated epileptic animals in this way, but he states that several American physicians have pursued the plan with benefit on the human subject He applies a solution of nitrate of silver (60 grains to ℥j aquæ) to the larynx every day, for five or six weeks

Fever, Continued—Dr Gee and Mr Eddowes (Lancet, July) have administered quinine in continued fever (apparently maculated typhus) in doses of from 3 to 10 grains, every two or three hours, in powder The average duration of the disease, if the treatment was commenced within the first seven days, was 4 4 days, if during the second week, 5 8 days The effect on the pulse was usually to cause a gradual and steady reduction in point of frequency

Dr Todd (Med Times & Gaz, August) has treated 18 cases of maculated typhus with repeated doses of brandy (½ to 1 oz every hour or half hour, day and night) In addition ammonia and chloric æther, and strong beef tea, were administered One case died, in the other cases, the benefit from the treatment was very marked

Fever Intermittent—Two years since, M Bartella brought forward his plan for the more economical treatment of ague, by administering equal parts of sulphate of quinine and tartaric acid He now (Bull de Thérap, tom xlv p 49) re enforces his former statements by new facts having treated altogether 208 cases in this way 196 of these being simple intermittents, and 12 pernicious fevers

Dr Bastille, too, who has been pursuing the same experiments states as his conclusions (Gaz des Hop, No 87)—1 That sulphate of quinine given with equal parts of tartaric acid is more active than the simple sulphate, 2 As a general rule, half the quantity of quinine so combined suffices, but in some descriptions of fever, as in the pernicious ones of Italy, larger doses than this are required

Furunculus—M Nelaton (Gaz des Hop, No 96) observes, that the development of furuncles may be always arrested by keeping the part covered with a linen compress which has been dipped in concentrated alcohol This must be accurately applied to the part, and care taken to keep it constantly moist, so that evaporation may be continually taking place from its surface

Mr Flint (Assoc Med Journ, July) recommended the application to boils and carbuncles of poultices and emollient remedies for twenty four hours, and then the application of strips of lead plaster to the boil and to the surrounding skin so as to produce a considerable degree of pressure, the plaster is to be changed daily, or every three or four days, according to the amount of discharge

Gleet—See Creosote

Insanity—Dr Oliver (Med Times & Gaz, July 16) recommends in mania and dementia large doses of opium and morphia He refers to patients who, at the time of this communication, were taking with benefit from 16 to 20 grains of solid opium daily, and narrates one interesting case at length, to show the value of the treatment

Leucorrhœa—*Menorrhagia*—See Creosote

Neuralgia—Ciasio (Schmidt's Jahrb, No 7, p 27) recommends the endermic application of atropine, 1 grain dissolved in alcohol is mixed with ʒj of lard, and applied to a blistered surface The belladonna soon produces its physiological effects, and sometimes these are intense In all cases, however, the neuralgia disappears, or is much lessened.

Ophthalmia Granular Lids—M Hirson, who so strongly praises tannin as the most valuable astringent application in granular lids and other affections of the eye, recommends the following formula as ensuring the equable diffusion and application of this substance, which, when not used in an impalpable state, may induce irritation—Tannin 5 parts, distilled water 20, dissolve, and add 1[illegible] of gum Arabic, and strain—See Creosote

Otalgia—M Delioux (Bull de Thér, tom xliv p 529) recommends in otalgia, and also in cases of tinnitus aurium arising from excessive sensibility of the nerve, that the vapour of ether should be conducted into *the meatus by means of a small tube* at the same time the hand is kept over the ear for five or six minutes Absorption rapidly takes place, and a cure is sometimes at once produced If necessary, the process may be repeated several times a day

Pertussis —Dr Fleetwood Churchill (Monthly Journal, August), following the advice of Dr Simpson, has used chloroform inhalations, not carried to narcotism, in several cases In the cases of very young children, it is difficult to cause the inhalations to be performed at the right time and in the proper manner, but in older children (of twelve or fourteen), Dr Churchill directs the inhalation to be used at the moment when tickling in the larynx is felt, the chloroform removes the spasm, and the cough is for the time prevented By persevering with the chloroform in this way, the threatenings of attacks become less frequent, and at last cease Dr Churchill does not use a handkerchief or an inhaler, but the patient is taught to drop twenty or thirty drops in o the palm of the hand, and to inhale the vapour mixed with a good deal of atmospheric air

Dr Watson (Assoc Med Journ, August) repeats the statement lately made as to the great utility of topical applications to the larynx in hooping cough * He uses the sponge and whalebone employed by Dr Green, with a solution of nitrate of silver, of *variable* strength (from grs xv to ℈ij to water ℥j), according to the stage of the disease, the weaker solutions being employed in the early and inflammatory stages The application is used every second day, or more frequently if the hoops are violent If the application causes violent vomiting, frequent small doses of heavy magnesia with a small quantity of trisnitrate of bismuth, are recommended Dr Watson has used the hyposulphate of soda and silver† with good effect, and promises a further communication on this subject

Phthisis —M Trousseau (L'Union Méd, Août 25) has revived a method of treatment proposed by Dioscorides—viz, *arsenical inhalations* He mixes one part of arsenite of potash with twenty of water and moistens with part of the liquid a piece of filtering paper When this is dry it is rolled into the form of a cigarette, and smoked once or twice a day, for one or two weeks The vapour appears to cause a good deal of irritation, and often after four or six whiffs the cigarette must be laid aside for a few minutes M Trousseau states that this plan diminishes the bronchial catarrh and the expectoration, but has no effect on the deposit of tubercles

Porrigo —See *Tinea*

Rheumatism —Dr Owen Rees (Lancet, June 11) refers to his recommendation of *lemon juice*, and states that although he considers it as 'an antidote to the true rheumatic diathesis," he has found it useless in gonorrhœal and syphilitic rheumatism, and in the rheumatic pains in Bright's disease, or in those connected with non gonorrhœal purulent discharges

Spina Bifida —M Chassaignac (Bull de Therap, xlv 63) strongly protests against the prevalent practice of abandoning cases of spina bifida as hopeless, for although many of these are accompanied by such organic changes as to render a cure impossible, others are placed in more favourable conditions, and call for attempts for their relief In proof of this he relates an interesting case, in a child five months old, in which a radical cure was effected by iodine injections

Strabismus —Mr Spencer Wells (Med Times and Gaz, August 27) speaks highly of the effect of prismatic spectacles On the sound eye the use of the prism produces an image on a point which does not correspond with that of the other eye, double vision is thus produced, which, if the prism be not strong, is corrected by increased action of an abductor or adductor muscle, as the case may be, which brings the image on to the corresponding point Advantage is taken of this in strabismus, and contraction of the relaxed muscle is caused In some cases the prismatic glasses were found very useful after operation, when by themselves they had been inefficient

Tænia —Dr Christison (Monthly Journal, July) employs with great success in tapeworm, the ethereal oleo resinous extract of the male shield fern (Lastræa Felix mas), in doses of 20 to 21 grains

---

* See No 23 Therapeutical Records -Pertussis

† See No 22, p 567

Tinea favosa—In a case of favus, Dr Jenner (Med Times and Gaz, August) has used with great effect a wash of *sulphurous acid* The acid is made by passing a stream of the gas through water to saturation, and of this strong solution one part is added to three of water Rags wet with the solution are applied over the head, and covered with oil silk The parasitic plant is destroyed, and the crusts rapidly separate, leaving a healthy or somewhat reddened skin, to which simple dressing may be applied

Tumour, erectile, of the Orbit—Dr Brainard (Lancet, Aug 20) relates a case of orbital tumour, unaffected by ligature of the carotid, partially improved by puncture with hot needles, but finally cured by the injection into the tumour of ʒj of lactate of iron (8 grs to ʒj aquæ)

Ulcers—Mr Spencer Wells (Med Times and Gaz, July 23) has treated ulcers by galvanism with great success He uses either two plates (zinc and silver joined together by a wire) or Pulvermacher's chain If the former be used, the silver plate is placed over the ulcer, the zinc plates on the moistened skin at some little distance Granulations form on the ulcer very rapidly, so that a great difference is often noted even in the short interval between two dressings The plates (or chain, if used) are kept constantly applied

Urine Incontinence of—In cases of involuntary micturition at night, Dr Deiters (Pr Ver Ztg, 1853, p 16) praises extremely the effects of cubebs, given in tolerably large doses twice a day for three to eight weeks The same remedy is useful in nocturnal seminal emissions

Uterus Prolapsus of—In cases of prolapsus, and in vesico vaginal fistulæ Mr Marshall (Med Times & Gaz, July 2) has obtained excellent results from cauterization with a wire heated by galvanism In prolapsus, a series of eschars are made by the same means in the mucous membrane of the vagina, and after cicatrization, the passage is considerably contracted

Worms—MM Beauclair and Vigner (Gaz Med, No 30), in the course of a paper having for its object to demonstrate that the production of worms must be regarded as a diathesis, and that in treating those suffering from the disease we must aim at the correction of the vitiation of the humours testify to the great benefit they have derived from the administration of cod liver oil They recommend the following formula—5 drachms of the oil are to be mixed with 6 of powdered sugar 15 grains of bicarbonate of soda, 6 drops of essence of mint, and 1 drop of the essence of bitter almonds This is to be given fasting, divided into two doses In the case of adults, the entire quantity is to be given, substituting carbonate of potass for the soda They at the same time recommend tepid alkaline baths for half or three quarters of an hour, and when the functions of the skin are slow in re establishing themselves, warm air baths, or baths giving off ammoniacal fumes, are useful Good diet, and all hygienic means calculated to fortify the cutaneous, respiratory, and digestive functions, are also indicated

POSTSCRIPT

We have received from Dr M'William a statement in answer to an article in the last number of the 'Edinburgh Review entitled 'Quarantine, Small Pox and Yellow Fever' We regret extremely that we are unable to publish Dr M'William's answer at present, but we hope to do so in a future number To those who have followed the progress of the discussion on yellow fever in our pages, it will be no surprise to hear that Dr M William has had no difficulty in showing, that of all the inaccurate statements which have disgraced the yellow fever controversy, this article in the 'Edinburgh Review is nearly the worst We must of course admit that the reviewer intended to state what he believed to be truth, but if so, we can only wonder at the violence of the partizanship which has so completely blinded and confused his judgment We cannot avoid expressing our great regret that the editor of so influential a work as the 'Edinburgh Review' should have published this article without putting it into the hands of some competent medical man, who might have laid before him the overwhelming array of facts which are disguised, distorted, or ignored in the mischievous pages of the 'Review'

BOOKS RECEIVED FOR REVIEW

The Retrospect of Medicine Edited by W Braithwaite Lecturer on Obstetric Medicine at the Leeds School of Medicine Vol xxvii January—June 1853

The Decline of Life in Health and Disease being an attempt to estimate the causes of longevity By Barnard Van Oven M D London 1853 8vo pp 300

Die Erkenntniss der Lungen krankheiten vermittelst der Perkussion und Auscultation Von Dr H Locher Zurich 1853 8vo pp 312

Notes on Pericarditis Endocarditis and Organic Disease of the Heart and Aorta as observed chiefly in the Jamsetjee Jejeebhoy Hospital at Bombay By C Morehead M D Professor of Medicine Grant Medical College

Vestiges of the Natural History of Creation Tenth edition Illustrated by numerous engravings on wood London 1853 8vo pp 325 with an appendix

Half yearly Abstract of the Medical Sciences Edited by W H Ranking M D Cantab and C B Radcliffe M D Lond L R C P Vol xvii January—June 1853

The Gulstonian Lectures On the Acute Specific Diseases By W Jenner M D Lond F R C P Professor of Pathological Anatomy University College &c London 1853 Pamphlet pp 55

The Pathology of the Bronchio pulmonary Mucous Membrane By C Black M D Edinburgh 1853 Part I 8vo pp 99

Lehrbuch der Pathologischen Anatomie und Diagnostik Von Dr Carl E Boeck Leipsig 1852 Zweiter Band

Reports by Neil Arnott Esq M D and Thomas Page Esq C E on an Enquiry ordered by the Secretary of State relative to the prevalence of Disease at Croydon and to the Plan of Sewerage 1853 Parliamentary Paper)

Report on the Cholera in Jamaica and on the General Sanitary Condition and Wants of the Island By Gavin Milroy M D 1853 (Parliamentary Paper)

Second Annual Report of the Wilts County Asylum for the year 1852 (with an Appendix)

Rapport à M le Préfet de Police sur la question de savoir si M le Dr Auzias Turenne peut etre autorisé a appliquer ou à expérimenter la Syphilisation à l Infirmerie de la prison Saint Lazare Par MM les Docteurs Melier Ricord Denis, Conneau et Marchal (de Calvi)

Verslag omtrent de Ziekten van het Hart en de Groote Vaten &c Den Dr F J I Schmidt Rotterdam, 1852

Commentaries on the Surgery of the War in Portugal Spain France and the Netherlands from the Battle of Roliça in 1808 to that of Waterloo in 1815 Revised to 1853 By G J Guthrie F R S Fifth edition 1853

Beitrage zur Kenntniss der Urinabsonderung bei Gesunden (Bestimmung der Phosphorsaure und der freien Saure im Urine) Inaugural Abhandlung Von August Winter

Habit Physiologically Considered (a Lecture delivered May 9th 1853 at the Bristol Literary and Philosophical Institution) By J Addington Symonds M D (From the Psychological Journal for July 1853) London 1853

Lehrbuch de Zoochemie Von H W Heintz Berlin 1853

Ueber Rheumatismus und Gicht Von Dr G F Wiss Berlin 1853

Sketch of the Operation and of some of the most important results of Quarantine in British Ports since the beginning of the present century By Gavin Milroy M D &c London, 1853

On the prevalent Treatment of Disease By Frederick C Skey F R S London 1853

On Dysentery By Arthur Dupuy M R C S L (Inaugural Dissertation) Edinburgh 1853

Recherches sur la Folie Paralytique et les diverses Paralysies Générales Par Jules Favrèt, M D Paris 1853

Der Harnstoff als maass des Stoffwechsels Von Th L W Bischoff Giessen 18 3

A Manual of Materia Medica and Therapeutics By J Forbes Royle M D F R S Professor of Materia Medica and Therapeutics in King s College London London, 1853 Second edition

Valedictory Address delivered to the Members of the Royal Medical Society of Edinburgh By W M Dobie M D Senior President of the Society Edinburgh 1853

An Account of the System of Clinical Instruction and Examination followed in the Grant Medical College at Bombay By C Morehead M D (Reprinted from the Seventh Report of the Grant Medical College)

An Essay on Organic or Life Force By J H Waters A B Philadelphia 1851

Burton and its Bitter Beer By J Stevenson Bushnan M D Fellow of the Royal College of Physicians at Edinburgh London 1853

Report of the Twenty second Meeting of the British Association for the Advancement of Science held at Belfast, in September, 1852 London 1853

INDEX TO VOL XII

BRITISH AND FOREIGN MEDICO-CHIRURGICAL REVIEW

PAGE

Abscess, mammary 276
Albumen, conversion into fat 39
  crystallization of 349
  in the urine 46
Alison, Dr Scott, on Applications to the Larynx 206
American Medical Association, Transactions of 453
Ancell Mr, on Tuberculosis 443
Aneurism, treatment of 278, 557
Arneth, Dr, on Midwifery 366
Arnott, Dr, on the Croydon Fever 402
Ascites, treatment of 278
Asthma, spasmodic, treatment of, 278, 558
Atrophy, pulmonary, as cause of heart disease 215

Bazin, Dr, on Tinea 416
Beale, Dr Lionel on Diabetes 226
Beneke, Dr, on Rational Medicine 61
  on the Whey Cure 149
Bennet, Dr, on the Uterus 22
Bibliography, Dr Day's Physiological 453
Bidder and Schmidt on Digestion 167
Bile, composition of 187
  uses of 190
Billing Dr, on Pulmonary Diseases 398
Bird, Dr Golding, on Urinary Deposits 123
Blood, abnormal conditions of 535
  Dr Thomas Williams on 249, 465
  crystallization of 349
  temperature of 253
Blood globules, development of 252
Blood letting, local 278
Bouchut, Dr, on Diseases of Children 108

PAGE

Brain, condition of grey substance of, from over work 551
Brain, Dr Brinton on 258
Breast, hydatids of 555
  inflammation of, Mr Nunn on 202
Bright, Dr, on Pancreatic Diseases 154
Brinton, Dr, Translation of Valentin's Physiology 201
Bronchitis, Dr Black on 537
Bubo, treatment of 279

Calcaneum, Mr Greenhow on Excision of 233
Callus, Dr Hiltz on 538
Cancer colloid 555
  eburne 543
Carpenter, Dr on Digestion 167
  on the Nervous System 1
Cataract, on operation for 274
Cell theory 285
Cerebral artery, blockage of 271
Chambers, Dr T K, Decennium Pathologicum 244, 487
Children, diseases of 108
Chloroform, poisoning by 558
Cholera, Asiatic, treatment by affusion 558
  albumen in the urine of 54
  Dr Mahony on 203
  Dr Stevens on 207
Chordee, treatment of 279
Chloroma 543
Chlorosis, employment of frictions in 277
Circulation the, rapidity of 256
Clinical Phrase book 201
Coffee, effects of 548
Constitution water 136

PAGE
Consumption, Dr Turnbull on 199
Cooper, Mr White, on Near Sight 204
Co operation, scientific 380
Cotton, Dr, on Tuberculosis 443
Creatine, crystals of 126
Creatinine, crystals of 127
Creosote, uses of, in bronchitis 558
Cretinism 119
Croup, treatment of 279
  Dr Weber on 271
Croydon, fever at, Dr Arnott on 462
Cryptogamia on the skin and mucous membranes 416
Crystallization, albuminous 349
Cysts, hæmorrhagic, of the neck 273

Degeneration, fatty 30
Decennium Pathologicum 244, 487
De Méric, Dr, on Syphilization 208
Deposits, urinary 123
Diabetes, composition of kidneys and liver in 226
Dietetics, Dr Moleschott on 401
Digestion, influence of pneumogastrics on 261
Dropsy, treatment of 279
Duncan, Dr Matthews, on the Uterus after Delivery 506
Dunglison's, Dr, Medical Lexicon 205
Dysentery, treatment of 279

Education at Oxford 81
Education physical, of Cretins 121
Elephantiasis Græcorum 434
Empyema, treatment of 279
Endocardium structure of 254
Epilepsy, treatment of 558
Epiphytes and Entophytes 416
Epithelioma 544
Erysipelas, albumen in the urine of 50
  treatment of 279

Fat, formation of, from albumen 39
Fatty degeneration of kidneys in diabetes 226
Fatty discharges in pancreatic disease 155
Fever, treatment of 280, 558
Fluids, Digestive, Bidder and Schmidt on 167
Foramen ovale, muscularity of its valve 255
Forbes, Dr, on Ireland 206

PAGE
Fracture, treatment of 280
Funis prolapsed reposition of 557
Furunculus, treatment of 559

Gairdner, Dr, on Dilatation of the Heart 209
Ganglia, sensory functions of 2
Gastric juice 178
Globules, compound, in urine 147
Gonorrhœa, treatment of 281
Greenhow, Mr, on Excision of the Os Calcis 233
Guggenbuhl, Dr, on Cretinism 119

Habit, Dr Symonds on 461
Headland, Mr, on Therapeutics 88
Heart, dilatation of, causes of 209
Heart, diseases of, Dr Chambers on 487
Heart, hypertrophy of, nerves of 256
Hemiplegia caused by blockage of artery 271
Heteropia, Dr Lebert on 539
Histogenesis Dr Lyons on 454, 532
Histolysis, Dr Lyons on 454, 532
Historic data on infantile pneumonia 514
Hæmorrhoids, treatment of 281
Hysteria, Dr Dalziel on 461

Iconography, ophthalmic, M Sichel on 208
Impregnation 266
Indigo in urine 145
Insanity, the theory of 424
  treatment of 281, 559
Intestines, obstruction of 315
Intestinal juice 193
Iodoform 281
Ireland Dr Forbes on 206
Irritation, spinal 25

Jenner, Dr, on Sarcinæ in the Ventricles of the Brain 530
John's, Dr Montgomery, Clinical Phrase book 201

Larynx, topical applications to 205
Leukæmia, Dr Griesinger on 269
  Dr Uhle on 535
  Dr Leudet on 536
Lime, oxalate of, in the urine 141
Liver, secretion of sugar by 551
Lyons, Dr, on Micrology 533
Luxations, reduction of 554

PAGE
M'Cormac, Dr, on sanitary reform 204
Mackenzie, Dr, on uterine disorders 22
Markham, Dr, his translation of Skoda 333
Medicines, action of 89
primary and secondary effects of 91
Menstruation, effect of, on milk 556
Meryon, Dr, on fatty degeneration 30
Micrology, Pathological Annals of 533
Midwifery in France and England 366
Moleschott Dr, on Dietetics 401
Moyse, M, on Pancreatic Diseases 154

Neuralgia, treatment of 559
Nervous system, functions of 1
Nerves, histology of 258
Nerve, phrenic 258
Norwegian Medical Journal 434

Obstruction, intestinal 315
Ophthalmia, treatment of granulated 559
Orthoscope 556
Otalgia, treatment of 559
Oxford, University of, Natural Sciences at 73

Pacchionian bodies 259
Paget Mr, on Surgical Pathology 454
Pancreas, diseases of, Dr Bright on 154
M Moyse on 154
Pancreatic juice, uses of 192
composition of 191
fluid, amount secreted 262
Paralysis, muscular atrophic 267
Pathology, Surgical, Mr Paget on 454
Percussion and Auscultation, Dr Skoda on 333
Perspirations, nocturnal, treatment of, 281
Pertussis, treatment of 281, 560
Phlebitis, cancerous 556
Phrenic nerve, Dr Luschka on 343
Phthisis, treatment of 281, 560
Placenta, fatty degeneration of, Dr Barnes on 30
Pleurisy, albumen in the urine of 57
Pneumonia, albumen in the urine of 56
intermittent 552
Prison discipline, Dr Faye on 439
Prostate enlargement, treatment of, 282
Pyæmia 269

PAGE
Quain, Dr, on fatty degeneration 30

Radesyge 440
Remedies, arrangement of 92
Renal casts, significance of 270
Respiration, movements of lungs and heart in 257
Rheumatism, treatment of 282, 560
Ricord, Dr on Syphilis 385
Robin, M, on Parasitic Vegetables 416
Rokitansky on fatty degeneration 30
Rubeola, treatment of 282

Saliva, composition of 167
uses of 171
Sarcinæ Goodsirii in the ventricles of the brain 530
Sarcina, treatment of 282
Scabies crustosa 547
Dr Boeck on 439
Scarlatina, treatment of 282
albumen in the urine of 51
Sciences, Natural, at Oxford 73
Sclerema 275
Secretion, manifestation of electricity in 263
Serosity, effusions of 554
Skoda, Dr, on Percussion, &c 333
Small pox and vaccination 463
Spedalskhed 434
Sperino, Dr, on Syphilization 393
Spermatorrhœa, treatment of 282
Stomatitis treatment of 282
Strabismus treatment of 560
Stricture of fundus uteri a cause of hæmorrhage 557
Syphilis and Syphilization 385
Syphilization, Dr de Méric on 203

Tænia, treatment of 560
Tea, effects of 549
Tilt, Dr on Diseases of Women 203
Tinea favosa treatment of 560
Tinea Dr Bazin on 416
Tonsillitis treatment of 282
Toynbee, Mr, on Artificial Membrana Tympani 460
Tracheotomy 271
Tubercle, Dr Van der Kolk on 196
Tuberculosis, Ancell and Cotton on 443
Tumours, Cystic, Dr Virchow on 540
Turnbull, Dr, on Consumption 199

PAGE

Ulcers, treatment of, 204, 205 286
Urates, amorphous composition of 132
Urea, crystallization of 125
  determination of 547
Uræmia 268
Urine, incontinence of 560
  albumen in 46
Urinary deposits 123
Uterus, anteflexion of 556
  condition of internal surface of, after delivery 506
  constitutional disorder of 22
  inflammation of 22
  prolapsus of, treatment 560
  vaginal injections in disease of 283

PAGE

Vaccination 463
Valentin, Dr, on Physiology 201
Van der Kolk, Dr, on Tubercle 196
Variola, albumen in the urine of 55
Vichy water, composition of 136
Vogel, Dr, on Scientific Co operation 380

Warts, treatment of 283
Water, action of 550
Whey Cure, Dr Beneke on the 149
Wiesbaden, water of 207
Williams, Dr Thomas on Blood 465
Willshire, Dr, on Infantile Pneumonia 514
Worms, treatment of 560

END OF VOL XII

Savill & Edwards, Printers, 4, Chandos-street, Covent-garden